SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

OPENING ADDRESS[1]

WE have come together especially to take note of the fact that fifty years ago a number of prominent workers in the field of science founded the National Academy of Sciences, receiving a charter from the United States government. It would be interesting and instructive to call the roll of the founders and learn who they were, but it will suffice to refer to some of the most conspicuous among these or, perhaps it would be better to say, some of those whose names are most familiar to the present generation. High up on this honor list are Louis Agassiz, James D. Dana, Wolcott Gibbs, B. A. Gould, Asa Gray, A. Guyot, Joseph Henry, J. Leidy, J. P. Lesley, Benjamin Peirce, R. E. Rogers, W. B. Rogers, L. M. Rutherfurd, Benjamin Silliman, Jeffries Wyman and J. D. Whitney. Fifty names are included in the act of incorporation. Among those are several members of the United States Army and Navy, as for example, J. G. Barnard, J. A. Dahlgren, Charles H. Davis, John Rogers, J. G. Totten, and others holding positions in the United States Military Academy and the United States Naval Observatory.

A careful scrutiny of the list of incorporators will show that they can be classified under three heads. The majority were engaged in scientific researches and had reached results of value. They were the leaders among the scientific investigators of that day. Then there were those who had gained distinction by their services as engineers either in the army or navy; and a

[1] Delivered by the president at the anniversary meeting of the National Academy of Sciences, April 22, 1913.

third class was composed of heads of national institutions such as the United States Naval Observatory, Naval Academy, Military Academy and Coast Survey.

Section 2 of the act of incorporation provides that the academy "shall consist of not more than fifty ordinary members," and that the academy "shall have power to make its own organization, including its constitution, by-laws and rules and regulations." Nothing is said in regard to the qualifications for membership. This is equally true of the constitution and rules except that Article I., Section 1, of the constitution requires that "members must be citizens of the United States." It should, however, be noted that Article IV., Section 4, of the constitution contains this clause:

> Each nomination shall, at the time of election, be accompanied by a written list of the original works of the nominee.

The reference is to nominations for membership, and the inference is clear that the nominee was assumed to have "original works" to his credit.

Whatever may have been the views of the incorporators, it has gradually come to be held that membership should stand for successful activity in the field of scientific research, the word scientific as here used meaning that which pertains to the natural sciences. But our predecessors did not intend to bind themselves to this meaning, as is clearly shown by the election of James Hadley in 1864, who, though a brilliant scholar, was certainly not distinguished for work in natural sciences; of G. P. Marsh in 1865; and later of Francis A. Walker and Richard Mayo-Smith.

As regards engineers who were prominently recognized in the early days of the academy the change of attitude that is worthy of notice is briefly this: While one who had accomplished some engineering feat was formerly regarded as worthy of membership by virtue of that fact, now the view appears to prevail that only such engineers as have advanced their subject by original contributions should be recognized.

And, finally, it is no longer held that the heads of scientific bureaus or departments of the United States government should necessarily be made members of the academy, no matter whether they have been actively engaged in scientific research or not. It is evident therefore that the field of choice has gradually become narrower.

What was perhaps regarded as the most important part of the act of incorporation is contained in Section 3 and reads as follows:

> The academy shall, whenever called upon by any department of the government, investigate, examine, experiment and report upon any subject of science or art, the actual expense of such investigations, examinations, experiments and reports to be paid from appropriations which may be made for the purpose, but the academy shall receive no compensation whatever for any services to the government of the United States.

This clause is still valid. The United States government may at any time call upon the academy for investigations, opinions and advice on any subject of science or art and this without charge for services.

In order the more clearly to understand the situation that existed in 1863 we should bear in mind two facts. First, there were at that time but few scientific bureaus forming part of the national government; and second, it was a time of war.

Perhaps it would be better to state these facts in the other possible order. Service to the government was uppermost in men's minds. If they could not help in one way, they could in another. What more natural than this willingness to place their knowledge and skill in scientific matters at the disposal of the government? This was an

act of patriotism, and patriotism was in the air. While engineers, astronomers and mathematicians were then well represented in the works of those who were serving their country in one capacity or another, it was a difficult matter for those in authority to secure authoritative opinions and advice in other branches of science. There was a gap to be filled. By granting a charter to a group of the leading workers in all branches of science on the terms under consideration the gap was filled in a most satisfactory manner. After that act there could no longer be excuse for not seeking scientific advice whenever it was desired or needed.

How did this work? An examination of the records shows that for a number of years after the National Academy was incorporated the government perpetually called for reports. Six such reports were made in the first year of the existence of the academy. The subjects were: "On the Protection of the Bottoms of Iron Vessels"; "On the Magnetic Deviations in Iron Ships"; "On an Alcoholometer"; "On the Explosion of the Boiler of the United States Steamer *Chenango*"; "On the Use of Aluminium Bronze for Cent Coinage"; "On Wind and Current Charts and Sailing Directions." In 1865 there were two reports; in 1866, four; in 1867, two, both of which are worthy of special mention. They are "On the Improvement of Greytown Harbor, Nicaragua," and "On Galvanic Action from Association of Iron and Zinc." In 1868 there were two. In 1870 there was one report "On the Protection of Coal Mines from Explosions by Electricity"; another "On Removal of Ink from Revenue Stamps"; and a third "On Silk Culture in the United States." In 1875 and 1876 there was only one each. Then in 1878 there were several important reports—six in all—among them one "On Proposed Changes in the Nautical Almanac"; another "On the Use of Polarized Light for Determining Values of Sugars"; another "On the Measurement of the Velocity of Light" and another "On the Preservation of the Writing of the Original Declaration of Independence." While there have been important reports on important subjects since 1878, it is undoubtedly true that of late years the academy has been called upon less frequently than in the early years. At first the officers of the national government took the matter seriously, and this was to the advantage of the country. But with the multiplication of scientific bureaus supported by the government the need of help from the academy has become less and it is true that some of the subjects already mentioned, and others not mentioned, could have been reported upon by one or another of the existing bureaus had they been in existence at the time. But, even as matters now stand, there is ample room for the kind of activity which was in the minds of the founders. Large questions of a scientific character present themselves from time to time, and it is hard to conceive of a better method of dealing with such questions than that under consideration. In this connection it should be borne in mind that advice, even good advice, is not always heeded. Indeed it may happen that it is treated almost contemptuously. This is well illustrated by an actual case which deals with an important governmental problem. Owing to its importance this case may well be treated of in some detail.

The sundry civil act, approved May 27, 1908, requests the National Academy of Sciences to consider certain questions relating to the conduct of the scientific work under the United States government, and to report the result of its investigations to Congress. In order that the subject may

be clearly understood the language of Section 8 of the act referred to should be quoted:

Sec. 8. The National Academy of Sciences is required, at their next meeting, to take into consideration the methods and expenses of conducting all surveys of a scientific character and all chemical, testing and experimental laboratories and to report to Congress as soon thereafter as may be practicable a plan for consolidating such surveys, chemical, testing and experimental laboratories, so as to effectually prevent duplication of work and reduce expenditures without detriment to the public service.

A committee was promptly appointed and that committee gave serious and prolonged attention to the subject. In due time the committee submitted its report to the council of the academy. The council having approved, the president transmitted the report to the Speaker of the House of Representatives and the presiding officer of the Senate. Everything was done in proper form. The president of the academy congratulated himself on the personnel of the committee which he had appointed, upon the report and upon the fact that the academy had performed an important duty and had been, as he thought, of real service to the national government.

It were well perhaps to close the account of the incident at this point, but unfortunately the moral would be lost, and the only object of telling the story at all is to point the moral. Well, what happened next? It is not necessary to go into detail. The result was humiliating to the committee that drew up the report. That report seems to have been promptly pigeonholed. It is certain that, so far as we have any information on the subject, it was not given serious consideration by Congress. And yet whatever may have been its imperfections that report represented the views of a group of eminent men of science who had devoted much time and thought to the study of the problem before them and who at the request of the President of the United States had been given every opportunity to learn the facts. Such an experience need not dishearten. The charter still holds good, and accordingly, the academy stands ready, whenever called upon by "any department of the government," to "investigate, examine, experiment and report upon any subject of science or art." As time passes it will come to be recognized more and more clearly by those in authority that the scientific method is the one most likely to lead to results of permanent value. Briefly defined, the scientific method consists in studying the facts and then drawing the most logical conclusion from these facts. It is most desirable that our government should utilize to a greater and greater extent this method which is free from partisanship and has only truth to serve. In the long run the influence of the National Academy upon affairs of government must be felt. Far-sighted statesmen must see and do see that it is well for the country to have a body of workers in the field of science connected in some way with the government, and the day will come when this will be recognized more clearly and more generally than it is to-day. The question is not what is best for the academy? It is, what is best for the country? May we not hope that in the near future Congress will see its way clear to emphasize the importance of the connection between the government and the academy by providing it with a proper home which can serve as a center of general scientific activity? This subject has again recently been brought to the front and there is a possibility that favorable action may be taken.

By an act of Congress approved June 20, 1884, the National Academy of Sciences was "authorized and empowered to receive

bequests and donations and hold the same in trust, to be applied by the said academy in aid of scientific investigations and according to the will of the donors."

The funds under the general management of the academy and their purposes will now be stated in their chronological order.

1. *The A. D. Bache Fund.*—This amounts to over $50,000. It was provided by the will of Alexander Dallas Bache, one of the charter members and the first president of the academy, who was for many years superintendent of the United States Coast Survey. The academy is trustee, and the income is applied to the prosecution of researches in physical and natural sciences.

2. *The Joseph Henry Fund.*—This fund of $40,000 was contributed by a number of friends and admirers "as an expression of the donors' respect and esteem for Professor Joseph Henry's personal virtues, their sense of his life's great devotion to science with its results of important discoveries and of his constant labors to increase and diffuse knowledge and promote the welfare of mankind." The income was to be paid to Professor Henry during his life, and after his death to his wife and daughters, and after the death of the last survivor the fund is to be delivered to the National Academy of Sciences, "the principal to be forever held intact and the income to be from time to time applied by the said National Academy of Sciences in its sole discretion to assist meritorious investigators, especially in the direction of original research." Happily this fund has not yet come into the possession of the academy. It is not necessary to remind this audience that Professor Henry was for years the secretary of the Smithsonian Institution.

3. *The J. C. Watson Fund.*—This amounts to $25,000, and was provided by the will of Professor J. C. Watson, a distinguished member of the academy, who died in 1880. The income "shall be expended by said academy for the promotion of astronomical science." It is also provided "that the academy may, if it shall seem proper, provide for a gold medal of the value of one hundred dollars, from time to time to the person in any country who shall make any astronomical discovery or produce any astronomical work worthy of special reward as contributing to our science." Five medals have thus far been awarded, the recipients being B. A. Gould, Edward Schönfeld, Arthur Auwers, Seth C. Chandler and Sir David Gill.

4. *The Henry Draper Fund.*—In 1883 Mrs. Henry Draper, widow of Henry Draper, late our honored member, presented to the academy a fund of $6,000 for the establishment of a gold medal to be awarded by the academy every two years to the individual in this or any other country who makes the most important discovery in astronomical physics. This fund now amounts to $10,000, as, in accordance with the wish of the donor, the income, above what was required to provide the Draper medals, was for a time allowed to accumulate and was added to the principal until this amounted to $10,000. At present the excess of income is available for purposes of research in the line of astronomical physics. The Draper medallists named in chronological order are S. P. Langley, E. C. Pickering, H. A. Rowland, H. K. Vogel, J. E. Keeler, Sir William Huggins, G. E. Hale and C. G. Abbot.

5. *The J. Lawrence Smith Fund.*—In 1884 Mrs. J. Lawrence Smith, widow of one of our honored members, presented to the academy the sum of $8,000, the object of the gift being to promote the study of meteoric bodies, a branch of science which Dr. Smith had pursued with marked suc-

cess. In accordance with the wishes of the donor it was decided that a gold medal to be given as a reward for original investigations would be most appropriate. Any excess of income above what is necessary for the striking of the medal "shall be used in such manner as shall be selected by the National Academy of Sciences in aid of investigations of meteoric bodies to be made and carried on by a citizen or citizens of the United States of America." Only one J. Lawrence Smith medal has been awarded. The recipient was H. A. Newton, "for the investigation of the orbits of meteors." The income has otherwise been used to aid investigations, especially those of Professor Newton.

6. *The F. A. P. Barnard Medal.*—This was provided for by the will of the late F. A. P. Barnard, one of the incorporators of the academy, and, at the time of his death the president of Columbia College (now Columbia University). The fund is controlled by the trustees of Columbia University. They "shall cause to be struck, with suitable devices, a medal of gold, nine tenths fine, of the bullion value of not less than two hundred dollars, to be styled 'The Barnard Medal for Meritorious Services to Science,' and shall publicly announce that a copy of the same shall be awarded, at the close of every quinquennial period . . . to such person, whether a citizen of the United States or of any other country, as shall, within the five years next preceding, have made such discovery in physical or astronomical science, or such novel application of science to purposes beneficial to the human race, as, in the judgment of the National Academy of Sciences of the United States, shall be esteemed most worthy of such honor." In accordance with these terms the academy has recommended to the trustees of Columbia University the award of the Barnard medal as follows:

In 1895 to Lord Rayleigh and William Ramsay "for their brilliant discovery of argon, a discovery which illustrates so completely the value of exact scientific methods in the investigation of the physical properties of matter."

In 1900 to Wilhelm Conrad Röntgen, "for his discovery of the X-rays."

In 1905 to Henri Becquerel "for his discoveries in the field of radioactivity."

In 1910 to Ernest Rutherford "for meritorious services to science resulting especially from his investigations of the phenomena of radioactive materials."

7. *The Wolcott Gibbs Fund.*—When Wolcott Gibbs, who was one of the incorporators of the academy and at one time its honored president, reached the age of seventy in 1892 a number of friends presented him $2,600 to establish a fund bearing his name, the income to be devoted to aiding in the prosecution of chemical research. Dr. Gibbs presented this fund to the academy, the income to be administered by a board of directors, who "shall have absolute and entire control of the disposition of the income of the fund, employing it in such manner as they may deem for the best interest of chemical science."

8. *The Benjamin Apthorp Gould Fund.*—In 1897 Miss Alice Bache Gould, daughter of the distinguished astronomer, Benjamin Apthorp Gould, one of the incorporators of the academy, who died in 1896, presented to the academy the sum of $20,000 as a memorial of the life work of her father, the income to be used "for the prosecution of researches in astronomy."

9. *The Cyrus B. Comstock Fund.*—This now amounts to something over $10,000 and is to be increased by accumulations of income until it reaches $15,000. A part of the income is to be used to provide "once for every five years a prize in money to the bona fide resident of North America, who,

not less than one year nor more than six years before the awarding of the prize, shall have made in the judgment of the trustees the most important discovery or investigation in electricity or magnetism or radiant energy."

This gift was received in December, 1907, and the first Comstock prize will be awarded at the present meeting.

General Comstock was a distinguished engineer, and a member of the academy. He died in 1910.

10. *The O. C. Marsh Fund.*—Professor Marsh, for twelve years president of the academy, died in 1899. He bequeathed the sum of $10,000 to the academy, "the income to be used and expended by it for promoting original research in the natural sciences." This fund has not yet become available.

11. *The Alexander Agassiz Fund.*—Alexander Agassiz, who was president of the academy from 1901 to 1907, died in 1910, and bequeathed to the academy the sum of $50,000 unconditionally. No decision has yet been reached in regard to the uses to which this fund is to be put.

12. *The Agassiz Medal,* which will be awarded for the first time this year, was provided for by a gift of Sir John Murray.

While this account may have proved tedious to some of you, it seemed necessary for the purpose of giving a correct impression of the work now being carried on. The academy has sacred duties to perform. It will soon devolve upon the younger members to see that these duties are conscientiously performed.

The constitution provides that the academy shall hold one meeting in each year in the city of Washington and another at such place and time as the council may determine. Whatever may be said of the duties of the academy as the scientific adviser of the government and as a custodian of trust funds, it must be acknowledged that it is through the agency of its regular meetings that its influence is mainly exerted. In this as in other matters, it is the subtle, the intangible, the spiritual that tells. Workers in the field of science are supposed by some, perhaps by many, to be incapable of recognizing the force of the intangible, and yet scientific work must inevitably lead to this recognition. It is impossible to weigh and measure the effect of the meetings upon those who take part. But that effect is felt none the less, and it is certain that those who attend are in the long run benefited—some in one way, some in another. This is not a subject that lends itself to profitable discussion. It may not be out of place, however, for one who has been a regular attendant for over thirty years to make public acknowledgment of the debt which he personally owes the academy for the opportunities it has afforded him of associating with and counting among his friends those whose earnest, honest work has been an inspiration to him and to the world. This association has been an inestimable privilege for which he is deeply thankful.

The work of the academy will continue; new and younger members will take up the work. Is it too much to hope that when the centennial anniversary is celebrated some of the members here present may be remembered as we to-day remember with gratitude the founders?

IRA REMSEN

THE RELATION OF SCIENCE TO HIGHER EDUCATION IN AMERICA[1]

THE half century which has elapsed since the founding of this academy has witnessed a radical change in the relations

[1] Address delivered before the National Academy of Sciences on the occasion of the semi-centennial celebration of its foundation.

between science and education. This change is equally marked in the professional training which prepares students for their several callings, and in the general training which prepares them for the duties and enjoyments of citizenship.

Fifty years ago the professional study of science in our universities was confined within very narrow limits—surprisingly narrow to those who see those places as they are to-day. There was no room for science in the schools of theology or of law. Schools of philosophy, in the modern sense of the word, had hardly developed. Even in schools of medicine, where the study of natural science in universities first gained a foothold, there was relatively little of scientific method, as we to-day understand the words, either in the teaching or in the study. There was much more learning of names of things than there now is, and much less learning of reasons of things; much more of tradition and much less of investigation. The anatomy and chemistry of the medical schools of those days were good sciences, as far as they went, but they generally did not go very far. As to the use made of science there is truth in the remark of one of my former colleagues that down to a recent day the three learned professions of theology, law and medicine had not advanced far beyond the old conception of the magic of the tribal medicine man, that the important thing for science to do was to find proper formulas of exorcism with which to banish evil spirits from their several realms of action.

Outside of the universities, a half century ago, things were little or no better. There was a small number of schools of engineering and a still smaller number of schools of chemical technology; but they did not form part of a large scheme of business training for the nation as a whole. Most of the engineers had learned their profession in the field; most of the technologists had learned it in the shop.

All of this has changed during the fifty years of the life of the academy, and changed radically for the better. Our universities have developed scientific study in all their departments, and especially so in their schools of medicine and philosophy. Side by side with these university schools or faculties there have grown up colleges of engineering and technology, sometimes in connection with the university, sometimes outside of it, which lay a scientific foundation for many a calling that only a few years ago was thought to need no scientific foundation at all. The world has found a place for the scientific expert in every line, and is inclined to regard as the best school, not the one that has the most students, not even the one that can give the best general education, but that which in the different lines can train and furnish scientific experts of the highest rank and most varied knowledge.

For civilized nations have at last come to the conclusion that the old supposed antagonism between theory and practise was a misleading conception, and that the habit of drawing a sharp line between the theoretical man and the practical man was a pernicious one.

Fifty years ago a man who had obtained all his knowledge of his business by his own experience was habitually proud of the fact; he was, as the phrase went in those days, a self-made man who spent most of his time in worshipping his creator. He counted it a matter of superiority that he knew nothing except what he had found out himself and taught himself. To-day it is recognized that every practical man can learn much from the theorist; that there is room for the application of scientific principles in every department of life;

that the farmer, the manufacturer or the merchant, no less than the engineer or the physician, must prepare to avail himself of the theory which has been built up by investigators, which has been taught in laboratories and incorporated in books, if he would bring his practise up to the needs of the time.

Of all the conquests of modern science, there is none which, in my judgment, is more remarkable or significant than this conquest of current business opinion. We no longer draw a distinction between learned and unlearned professions. We have recognized that every profession and every trade, in order to be pursued to the best advantage, must be a learned one. None so complex as to be unable to get help from science; none so simple as not to need it. We have shaped our system of technical training accordingly; and we have learned to rate at their true worth the men and the places that can give training as research institutions, side by side with universities which make progress in such training possible.

Equally important, though of a different and perhaps less satisfactory character, has been the change in the scheme of our general education; in the choice of subjects and methods of teaching offered in preparation for the work of citizenship as distinguished from the preparation of each man for his business or calling.

The old course of study in our high schools and colleges consisted chiefly of classics, mathematics and metaphysics, with a little history and a few descriptive courses in natural science. Of scientific training in the modern sense it gave none, except to the unusual man whose mathematical tastes made the study of algebra and analytical geometry a means of scientific education in spite of text-book and instructor and class-room atmosphere, or the still more unusual man who used his grammar and metaphysics as an exercise in closely ordered reasoning. The course, as a whole, was constructed for the student whose interests were in the past rather than in the present and the future. The training which it gave—good, in many respects—was a training in memory, in expression and in accuracy of apprehending language, one's own or another's, rather than in scientific method, as we understand it to-day.

There is on the facade of the main hall of a university which has done much for education in many lines, a representation of philosophy in a dominant central position—old-fashioned metaphysical philosophy—with the different sciences laying tribute at her feet. I suspect that this is not an unfair characterization of the views as to the place of science in education which prevailed among most college faculties a generation or two ago.

Now let me say right here that I do not for a moment overlook the advantages of the old system. It taught the boys to use books and find things out from books, and to expect to do hard work for that purpose instead of to have somebody else make it easy. This was a great merit, and the boys trained under the old system showed this merit. But college faculties were often blind to the particular kind of book learning that was most significant for human progress and which was of most concern to the living world outside.

For at the time when the academy was founded, and in the time since, chemistry and physics and geology and biology were becoming not only matters of importance to the experts in their several callings, as I have indicated, but subjects of real and dominant interest to intelligent men who were not experts, but who cared for knowledge and who cared for current history.

A large section of the world, an increasingly large section of the world, cared more for books that explained the tendencies of the present than for those that embodied the ideals of the past. Perhaps this movement may have gone too far and may have caused people to care too little for the ideals of the past, to overvalue scientific reading as compared with historical or literary reading. I shall not try to discuss whether it did or not. At any rate, a curriculum which was exclusively occupied with classics and philosophy ceased to meet the demands of grown men or the needs of boys, and the course of study in our colleges had to be remodelled accordingly. Each decade of the last fifty years has witnessed a gradual crowding out of classics from our older schools and colleges by subjects of new and more present interest, and a growth of new schools and colleges of a different kind, where science in varying forms is made the chief subject of attention and other matters relegated more or less to the background.

Now this increasing interest in science is a matter about which we all, members of the academy and guests of the academy, scientific men and literary men, may rejoice heartily. But how far the things that are called science always deserve the name of science, or how far the teaching of such subjects by present methods always deserves the name of education, is quite another question. Every school superintendent likes to stimulate the attention of his pupils by giving them the opportunity to see amusing phenomena with their own eyes, and if possible set them in motion with their own hands. Under some circumstances this may be the best kind of scientific training; under other circumstances it may be no training at all.

Nature study—to quote a phrase which is popular among educators of the present time—is good if it is made the basis for teaching scientific methods, and bad if it is simply made a means of momentary amusement. Unfortunately, a large part of our school committees and school teachers think that the subject makes the science. They may not go as far as the author of "Murray's Handbook to Spain," who says that the mountains of that country, to quote his own words, "abound in botany and zoology." But they are apt to assume that the picking to pieces of flowers is in itself botany, and that hearing a carbon disulphide mixture make a loud explosion marks progress in chemistry, and to act accordingly. A short time ago a school superintendent in one of our more newly developed parts of the country said he had to make a change because it was so easy to find thoroughly competent teachers of physics, but that so few of them ever knew any algebra.

Fifty years ago the members of the National Academy of Sciences who held seats in college faculties were occupied in protecting science against its enemies. I am not sure but what to-day their chief duty lies in protecting it against its friends. When the National Educational Association says that high schools should be allowed to omit the study of algebra and geometry and that the colleges should be compelled to accept for admission an equivalent amount of "science"—God save the mark!—it is time for the true friends of science to call a halt.

For the importance of scientific training to the student in our high schools and colleges is not due primarily, or in large measure, to the facts of physics or biology that he learns in the school. It is due to the training in certain habits of observation and deduction, in certain methods of hypothesis and verification, which he can get more effectively by a good course in science

than by one predominantly devoted to languages, where the scientific training is merely incidental. That the facts of physics or biology are more interesting to the student and to the world than those of Latin and Greek and have more obvious bearing on everyday life is a help to the teacher in securing the voluntary cooperation of the pupil; but it is far from being the fundamental reason why the subjects themselves are educationally valuable. It is not the subject that makes the course scientific; it is the method.

You have been good enough, Mr. President, to refer to my father's connection with the academy, and I for my part am glad to take the opportunity to say that he regarded his election to membership in this body as the greatest honor he ever received. I feel sure, therefore, that I shall be pardoned if I illustrate the point I have just made by reference to my father's teaching.

Fifty years ago the one course in the academic department of Yale College where modern science was really taught, was the course in freshman Greek. For my father, though he had the highest enjoyment of classical literature, was by training and temperament a philologist; and he taught the freshmen who came under him to take Greek verbs to pieces and compare and observe their parts and put them together again, and see what principles were involved in the analysis and synthesis, exactly as the botanist might have done with his plants or the chemist with his elements.

In those days chemistry and physics were taught in Yale College, as distinct from the Sheffield Scientific School, solely by text-books and lectures. Philology was taught by the laboratory method; and for that reason the freshman Greek course was a course in modern science and meant that to the pupils. The courses in chemistry and physics widened the boy's knowledge of facts and doubtless encouraged many of them to get scientific training for themselves afterward; but the course in freshman Greek was a course in science, because the boys learned to do the things, both easy and hard, which are the heritage of the man of science.

Science is not a department of life which may be partitioned off from other parts; it is not the knowledge of certain kinds of facts and the observation of certain kinds of interest, as distinct from other facts and other interests; it is a way of looking at life and dealing with life; a way of finding out facts of every kind and dealing with interests as varied as the world itself,

> Where each for the joy of the working, and each
> in his separate star,
> Shall draw the thing as he sees it, for the God of
> things as they are.

Arthur T. Hadley

Yale University

SPEECHES AT THE ANNIVERSARY DINNER OF THE NATIONAL ACADEMY OF SCIENCES

SPEECH OF THE RIGHT HONORABLE JAMES BRYCE

Doctor Woodward, President Remsen and gentlemen: I am very much touched by the kind words in which my old friend, Dr. Woodward, has introduced me to you, and I am more than grateful to you for the way in which you are kind enough to receive me. It does make one happy to be so received and to be assured that one has not lived in this country six years without having acquired some friendliness on the part of its people.

But, apart from that, gentlemen, I stand before you this evening as a rather unhappy man, because it is the last evening on which I am likely to have the privilege —at any rate, in an official capacity—of

meeting an audience of American men of science.

One of the most delightful parts of my sojourn in Washington has been my intercourse with your men of science. There is not any city in America—I doubt if there be any city in Europe—where so many men of eminence in science are assembled as live in Washington, and the gatherings which you have here, when the men of science from the whole of your wide country come together, have been among the most delightful experiences that I or any Briton has had in this country. I have had it also in Philadelphia and in New York, and I have had the pleasure of making the acquaintance of your men of science in many journeys all over the country; but this, after all, is the focus to which is gathered most of the scientific lights and leaders of the United States at stated intervals when you come together here.

And I can assure you, gentlemen, there is nothing I shall look back to with more pleasure, in so much of life as remains to me, as to the friendships I had formed with your scientific men and the inspiration I have derived from the ardor and energy with which they pursue the studies to which we are all so much debtors.

Dr. Woodward has suggested that I should say something about foreign academies, but my knowledge about foreign academies is, really, practically confined to my own country, for, whenever I have traveled abroad, it has rather been among the historians than among the men of science that my work has lain.

However, I should in any case feel a little doubtful about venturing to talk about scientific academies, knowing that, whatever else "science" means, Mr. Vice-President, it is supposed to mean knowledge; and if a man feels that he does not know a thing, scientific people are the last to whom he should address his remarks.

I received at Oxford my literary education, and I remember "education" being defined by a very eminent professor there, who said: "What our Oxford education does is to teach our men to write plausibly about subjects they do not understand"—an art which we were in the habit of exemplifying by immediately beginning to write for the journals and reviewing books—whose authors knew infinitely more about their subject than we did—in a very superior manner, an experience which, however, is not confined to England.

The vice-president said, gentlemen, that he regarded men of science with fear and veneration. I share those feelings. I have veneration for the lofty and disinterested spirit which you bring to your work. I have fear for the enormous power you exercise.

You are really the rulers of the world. It is in your hands that lies control of the forces of activity; it is you who are going to make the history of the future, because all commerce and all industry is to-day, far more than ever, the child and product of science; and it is you who make these discoveries upon which, when they are applied by industry, the wealth and prosperity of the world depend. It is in your hands that the future lies, far more than in those of military men or politicians.

But I have another feeling besides fear and veneration. It is that of envy. I envy you your happy lives. Compare your lives with the lives of any other class. If the vice-president will permit me, I think the life of a man of science is a great deal happier than the life of a politician or the life of a statesman, who, as we know, is many pegs above the politician, because the politician is occupied, as the vice-president has said, in endeavoring to promote the

interests of his party and not the interests of his country; and I discovered, during my experience in the House of Commons in England, that a legislative assembly is the worst place in the world for the discovery of abstract truth.

Or, take the case of the lawyer. So far from seeking to discover the truth, in one half of the cases which he conducts, he is endeavoring to obscure the truth. Or, even take the case of the artist or the literary man, who has a subject to work upon, delightful and interesting in itself, in evoking from the stone, or by colors, shapes or forms of beauty, which will far outlive him; but these forms of beauty will profit him very little if they do not commend themselves to the popular tastes, and he is constantly under the temptation of doing something less good than he wishes, in order to meet the tastes of his patrons.

It is the man of science who has the really happy life. He is engaged in the discovery of the truth, and nothing but truth. The applause of the multitude is nothing to him. He is working for a mistress more exalted than any popular assembly or any multitude that we can conceive of. He is working for Truth herself, and for the future. He is consecrating his efforts to the highest task that God can lay before man, and in that he needs nothing but the sense of what he is adding to the sum of human knowledge, and he has the incomparable pleasure of feeling that the more he knows, the more the immense ocean of knowledge stretches itself out before him. The further he outlines any path into the untrodden solitudes of ignorance, and the more he blazes those paths and makes them paths of knowledge, the more he sees other paths branch out before him, leading further and further away into the realms which others after him will traverse.

In these things, friends, there are elements of pleasure and delight, elements also of independence, which I think no other profession can equal. I was tempted to add one other charm which your life has. It is the charm of poverty. I have sometimes felt inclined to wish, Mr. Vice-President, that Congress was a little more liberal to the scientific men who are working for Uncle Sam. But perhaps they are to be congratulated on being free from those temptations which beset wealth. Poverty, like other things, is good if you have not too much of it, because it saves one from the temptation of forgetting the end to the means, the temptation to which most of us, and, above all, those who are in search of wealth, succumb. You keep the end always before you, and you proportion your life to that end.

Still, I think you might, with advantage, not only to you, but, what is far more important, to the whole country—and it ought to be possible in a wealthy country like this—provide upon a more ample scale for those who follow science, and give science a more exalted position, by freeing the scientific man from any thought of domestic anxiety.

You enjoy in this country—I speak here of particular branches of science—some things which we, in England, greatly envy. Think of what the geologist or the botanist has before him here! We have been working for one hundred and fifty years upon the geology, and for more than that upon the botany, of our little island; but here you have the whole continent open to you, and any man of science on these subjects can make a reputation for himself by new work in new fields, such as is impossible for us in outgrown Europe.

Gentlemen, one word I venture to say about the scientific bodies of the continent of Europe. We have, in the Royal Society, the oldest of those bodies, and one which, I think, has always maintained the level

which it took in the great days when Isaac Newton was one of its members; and now there has sprung up all over Europe a host of other bodies pursuing science and following it into those infinite ramifications which modern science has discovered. Everywhere there you are welcome. One of the most delightful things of science is that it knows no divisions or allegiance to nationality. It is a republic in which there is no passport to greatness, except service and genius, and it is a republic of which every one is a citizen, and where every one has equal rights in every part of the world.

That has always been our tradition in England and in our Royal Society; and I know it is your tradition here, and I know what hearty welcomes you have always given to our men of science when they have come over here, and how refreshed and invigorated in spirit they have been when they have gone back to their own country.

Gentlemen, I can wish nothing better for any of us than that these comings and goings will be frequent, and I can assure you that it will always be a pleasure to the scientific men of England and Scotland to welcome you to their societies and to all their gatherings and universities. I hope that, more and more, these meetings will take place, and I can assure you that all you have achieved and all that you are achieving in so many ways on so many different lines for the advancement of knowledge, for the extension of human power that comes through knowledge, is followed with gratitude and admiration by the scientific men of Great Britain.

SPEECH OF DR. S. WEIR MITCHELL

Mr. President, Mr. Vice-President, my brothers of the Academy: I am, I presume, the victim of the after-dinner hour, as usual, and am well aware of the treachery of the tongue, and much prefer the loyalty of the pen. I have, therefore, deliberately put on paper that which I want to say to you to-night, feeling that it will be much more probable that I shall interest you than if I trusted to my unassisted words.

I am, I presume, indebted to the liberal forbearance of time for the honor of being asked to speak to you this evening. It does not find me in the careless mood of after-dinner gaiety, nor is it possible to escape altogether from personal remembrances, which elsewhere than at this friendly board might entitle me to be relegated to what Disraeli called the "fatal time of anecdotage."

My diploma is dated August 25, 1865, three years after our foundation. It is signed by Dallas, Bache, Wolcott Gibbs and Louis Agassiz. Since then, one hundred and thirty-six of our fellowship have come and died, with an average duration of academic life of more than eighteen and a half years—very many with far less. This makes clear that in those earlier years our additions were of men older than those we elect now.

At present the liberal endowment of research opens the way to distinction for younger men, unembarrassed by the time-killing need to preach science as well as to practise it.

Between the mere words of our record—*elected—deceased*—you, who are familiar with our history, may read much that is written clear on the roll of scientific achievement.

Here are they to whom, from the depths of space, were whispered in the night watches its long hidden secrets. There, too, are those who, in the silence of the laboratory, rejoiced in the fertile marriage of the elements, or they who, like confessors, heard from dead bones or rock or flower

the immeasurable history of the silent ages of earth.

One might linger long over many of these lives whose interests were so remote from thought of the commercial gains their unselfish work made possible. But there are other compensations, and there are men here to-day who are aware that there is no earthly pleasure more supreme than to find disclosed some secret of nature unknown before, save to Him who set in motion the complex mechanism of the universe.

The later life of the merchant and the lawyer loses vitality of normal interest as age comes on; not so the man of science. The eternal love of nature is his—a mistress of unfading charm.

I remember once that, at my table, some one asked that ever happy naturalist, Joseph Leidy, if he were never tired of life. "Tired!" he said, "Not so long as there is an undescribed intestinal worm, or the riddle of a fossil bone, or a rhizopod new to me."

My first remembrance of an Academy meeting is of 1866. We met in a Smithsonian room. There were not more than fifteen present. Professor Henry was in the chair.

I remember Benjamin Peirce, Wolcott Gibbs and Gould. Agassiz sat on one side of me, and on the other Coffin. It was all very informal. The first scientific paper was by Professor Peirce, who for twenty minutes occupied us with algebraic formulas and mathematical figures, until he turned and said that he had got out of the region of material illustration, and so led us on through the endless equations in which I had lost myself at the very outset.

Agassiz turned to me at the close and said, "Were you able to follow him?" I said, "No; I can not do a sum in the Rule of Three without trying it over two or three times." Upon which the delighted naturalist added, "Ni moi non plus." Professor Coffin remarked, "He was traveling with Seven-league Boots over a country across which I should have to crawl."

Some of this was quite audible to Peirce, who said that the only thing required was more careful attention than men were willing to give to the great science of mathematics, and that our incapacity to understand and follow him was due to our want of proper education.

He was succeeded by Agassiz, who made the first announcement of his discovery of the additional heart found in the tail of the young of the salmon.

I recall very little else about these delightful people, except that they—all of them—were not only in the peerage of science, but also companions as socially interesting as they were learned.

Perhaps the most pleasant remembrance I have of all is of Louis Agassiz and Joseph Henry. The former was good enough to take a great interest in some of the animal physiology with which I occupied the rare leisure of a hard-worked young doctor. His enthusiasms were shown in odd ways at times.

On one occasion he was staying with Professor Frazier, and dismissed me on the front step one slippery day in February. I had got some distance from him when he came after me in haste, sliding over the pavement. "I did want to say to you one thing. Are you acquainted with the opossum?" I said, rather confused, "No." He said, "I advise you to acquire a physiological friendship with the opossum. He is a mine of physiological wealth."

Jeffries Wyman, who was elected in 1872 and died in 1874, was another who held a place in my most honoring regard. He resembled Joseph Leidy in that splendid magnanimity and unselfishness which con-

trasted so agreeably with the disgusting quarrels, happily rare, which sometimes arose among men of science.

As you have made me speak here, I am forced to say something of myself, and hence this anecdote of Wyman. I had written him word of the discovery I had made of the chiasm of the superior laryngeal nerves in the chelonia—that is to say, turtles—and it greatly excited him, especially my prediction that it would be found in serpents and probably in birds. A year afterwards he sent me a large bundle of illustrations and descriptions of what he had found in other classes than the turtle, and insisted that I should use them in the second paper I was about to print, stating that they would not have been discovered had it not been for my predictive aid. Of course, I declined this help; but it was characteristic of the noblest form of the scientific mind.

You will, I trust, pardon me if I close this long talk with a few too personal words about the much loved first director of the Smithsonian Institution, first of the men who sacrificed to that Institution a scientific career. When a boy about fifteen years old, I was sent by my father to Professor Henry at Princeton with some glass apparatus, which could not otherwise be sent without danger of breakage.

He met me at the station, took me to the house, and spent a part of the next morning endeavoring to explain to my bewildered youth the experiments he was making in the transmission of electric signals. I was overcome by the unwonted attention paid to a boy of my age, and expressed myself so warmly that he laughed as he bade me good bye, saying: "Well, life sometimes gives one a chance to return little favors, and perhaps some day you will have an opportunity to oblige me."

Long years passed by, and some time in the beginning of 1878 Professor Henry asked me to come to Washington and advise him. After a thorough examination of his case, he asked me plainly if he was mortally ill. I said, "Yes." Then he asked how long he had to live, but I could not set a date. He said, "Six months?" Hardly, I thought. He died in May of that year.

As I arose to go away, his carriage waiting, he said: "I have yet to discharge my material obligation. How much am I in debt to you?" I replied, "You are not in debt. There are no debts for the Dean of American Science."

He was much overcome, and said: "I have always found the world full of kindness to me, and now here it is again." I could only say: "You do not remember, sir, that once you said to me, a boy, when you had been very kindly attentive to me and I tried to express my obligation, that perhaps a time might come when I could oblige you. If this obliges you, my time has come." And so we parted.

I may add what some of you already know, that Alexander Agassiz wrote me he had intended to return home early from Europe, in order to give a dinner such as we are having here to-night. He died on the way over, and his letter reached me after his death—strangely enough, the fourth letter I have received from men who were not alive at the time their words reached me.

My talk has been of men dead long ago, but I should be ungrateful to the longest friendship of my life if I did not pause to remind you of our latest loss in John Shaw Billings. He was a man of too many competencies to allow of even allusive comment here. Few men have been better loved or had so enviable a capacity to convert mere acquaintance into friendship.

It is difficult for a man as old as I am to talk in the gay after-dinner mood, and

if I have been too somber and too personal, I trust that I may not have been guilty of the social crime of having been uninteresting.

SCIENTIFIC NOTES AND NEWS

ON the occasion of the installation of the Duke of Northumberland as chancellor of Durham University honorary degrees were conferred on the following men of science: D.C.L., Lord Rayleigh; D.Sc., Sir Archibald Geikie, K.C.B., Sir William Ramsay, K.C.B., Sir T. C. Allbutt, K.C.B., Sir J. A. Ewing, K. C. B., Sir William Crookes, O.M., Sir J. J. Thomson, O.M., and Professor E. B. Poulton.

THE Linnean Society, London, has awarded its Linnean medal to Professor Adolf Engler, of Berlin.

THE French Academy of Moral and Political Science has elected M. Pierre Janet, professor of experimental psychology at the Collège de France, to the chair left vacant by the death of M. Fouillée.

WHEN the Lobachewski Prize was recently awarded to Professor Schur, of Strasburg, the committee also awarded an honorable mention to Professor Julian L. Coolidge, of Harvard University, for his book on "Non-Euclidean Geometry," Oxford, 1909.

THE annual meeting of the Iron and Steel Institute was held in London on May 1 and 2, when the Bessemer gold medal for 1913 was presented to Mr. Adolphe Greiner by the president, Mr. Arthur Cooper. The Andrew Carnegie gold medal for 1912 was presented to Dr. J. Newton Friend.

THE Manchester Literary and Philosophical Society has nominated Sir Thomas H. Holland, F.R.S., to represent it at the twelfth International Congress of Geology, to be held in Toronto in August next.

DR. JAMES W. GLOVER, professor of mathematics and insurance at the University of Michigan, has been appointed expert special agent of the Bureau of the Census to supervise the preparation of a special volume on vital statistics. Extensive mortality tables are to be prepared, based on the population and vital statistics of the United States. Dr. Glover has also been appointed collaborator to the Office of Public Roads in the Department of Agriculture to assist in the preparation of several bulletins on the various methods of issuing and financing public highway bonds.

PROFESSOR AMOS S. HERSHEY, head of the department of political science at Indiana University, and Dean Walter Williams, dean of the school of jurisprudence at the University of Missouri, have been appointed fellows of the Kahn Foundation for the coming year, and both will begin a one-year tour around the world within the month. The fellowships carry with them a stipend of $3,000 for each appointee and in addition there is an allowance to each of $300 for purchases.

PROFESSOR HERBERT R. MOODY, of the College of the City of New York, is in residence for the summer term at Oxford University, where he is associated with Mr. T. V. Barker, of the department of mineralogy at the University Museum. Professor Moody is engaged in learning from Mr. Barker the details, so far as developed, of the von Federon method of Crystallo-Analysis. Mr. Barker learned Russian last year in order to work with von Federon in St. Petersburg. The method is not yet made public.

AT the annual general meeting of the Marine Biological Association of the United Kingdom, held in the rooms of the Royal Society on April 30, the following officers and members of council were elected for the year: *President,* Sir Ray Lankester; *Chairman of Council,* Dr. A. E. Shipley; *Hon. Treasurer,* Major J. A. Travers; *Members of Council,* E. T. Browne, L. W. Bryne, Dr. W. T. Calman, Professor H. J. Fleure, Professor F. W. Gamble, Sir Eustace Gurney, Commander Campbell Hepworth, Professor J. P. Hill, E. W. L. Holt, Professor E. W. MacBride, H. G. Maurice, Dr. E. Schuster, G. W. Smith, Professor D'Arcy W. Thompson; *Hon. Secretary,* Dr. E. J. Allen. The following governors are also members of council: G. P. Bidder, the

Earl of Portsmouth, Sir Richard Martin, the Hon. N. C. Rothschild, Professor G. C. Bourne, Dr. A. E. Shipley, Professor W. A. Herdman.

As has already been announced, the committee of the Lister Memorial Fund proposes that the memorial should be of a threefold character: (1) A simple marble medallion bearing a sculptured portrait of Lord Lister to be placed in Westminster Abbey among the monuments of the nation's illustrious dead; (2) a larger and more conspicuous monument to be erected in some public place in London, the city wherein he lived and worked; (3) if funds sufficient shall be obtained, the founding of an International Memorial Fund from which either grants in aid of researches bearing on surgery or rewards in recognition of important contributions to surgical science shall be made, irrespective of nationality. The *British Medical Journal* says that the sum already subscribed or promised is perhaps sufficient for the completion of the first two parts of the proposed memorial, which are of local character, but for the third, or international part of the memorial, an international appeal is now being made, and a letter has been addressed by the secretary, Sir John Rose Bradford, to the principal universities and medical societies on the Continent of Europe and in the United States. A similar letter is being sent to the corresponding institutions in the British dominions and colonies. Among the subscriptions received for the international fund are the following: Academy of Sciences, Paris, 500 francs; University of Paris, 500 francs; Medical Faculty of the University of Montpellier, 250 francs; the Karolinska Medico-Kirurgiska Institutet, Stockholm, £5. Vladimir Kowalevsky, president of the Technological Society of Russia, has made a donation of £5 "in memory of one of the greatest benefactors of the human race." The universities of Michigan, Yale, Harvard and Leland Stanford (California) have already undertaken to promote the memorial, and the College of Physicians, of Philadelphia, has made a special appeal to its fellows. In addition, Dr. Keen, of Philadelphia, is making a somewhat wider appeal. The University of Toronto has appointed a special committee to promote the fund. The honorary treasurers of the fund are Lord Rothschild and Sir W. Watson Cheyne. The offices of the fund are at the house of the Royal Society, Burlington House, Piccadilly, London, W., and subscriptions, made payable to the fund, may be sent to the treasurers there.

Congress has furnished the Bureau of Entomology with funds to be used in the eradication of the tick which transmits spotted fever in the Bitter Root Valley in Montana. Dr. T. Ricketts demonstrated that the disease is transmitted only by the tick *Dermacentor venustus.* Investigations conducted by the Bureau of Entomology in cooperation with the Montana Agricultural College have shown that a comparatively simple and inexpensive plan of eradication of the tick may be put into operation. Cooperation has been arranged with the Montana State Board of Entomology, recently created at the session of the Montana Legislature, whose duty it is to "study the dissemination by insects of diseases among persons and animals, said investigation having for its purpose the eradication and prevention of such diseases." The board is further required to take steps to eradicate and prevent the spread of diseases that may be transmitted by insects and an appropriation of five thousand dollars a year for the next biennium is made. The immediate object in passing the law was to provide for the eradication of the Rocky Mountain spotted fever tick. The membership of the board is *ex officio* and is made up of the secretary of the State Board of Health, chairman; the state entomologist, secretary; and the state veterinarian.

At the recent meeting of the National Association for the Study and Prevention of Tuberculosis in Washington the following resolutions were adopted: "Whereas, Widespread publicity has been given to the claims of an alleged cure for tuberculosis. *Resolved,* That there is no information before the National Association for the Study and Prevention of

Tuberculosis to justify the belief that any specific cure for tuberculosis has been discovered which deserves the confidence of the medical profession or the people, and, *Resolved,* That it is the duty of the public to continue unabated all the present well-tried agencies for the treatment and prevention of tuberculosis."

THE trans-Saharan party of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington arrived at Timbuctoo on May 16. Since leaving Biskra, Algeria, on October 29, 1912, the party has secured complete magnetic observations at about seventy stations.

MR. CAWTHRON, of Nelson, New Zealand, has offered to give $60,000 for a solar physics observatory.

THE Historical Medical Museum, organized by Mr. Henry S. Wellcome, which is to be opened in London towards the end of June next, will include, as we learn from *Nature,* among the exhibits in the science section a large collection of the original apparatus used by Galvani in making his first experiments in galvanism in the eighteenth century. Other exhibits will be a collection of votive offerings for health, ancient microscopes and optical instruments, amulets and charms connected with English folk medicine, early medical medals and coins from the Græco-Roman period, and early manuscripts and medical books.

THIS week a joint meeting of the Institution of Electrical Engineers of London and the Société Internationale des Electriciens is to be held in Paris.

THE Index of Authors and of Subjects in the first thirty volumes of the *American Journal of Physiology* is ready. The index contains about 160 pages and is bound in paper. The edition is limited. Such indexes are valuable to scholars, even to those who do not possess a file of the *Journal.*

UNIVERSITY OF SOUTHERN CALIFORNIA students have completed the organization of the first Pacific Coast chapter of the Agassiz Association, the national organization of nature students, choosing Burbank Chapter for their distinctive name. Mr. Luther Burbank will probably be one of the speakers to address the chapter this year. The chapter programs include bi-weekly field trips to points of interest around Los Angeles, frequent social gatherings at the homes of members and monthly lectures by outside naturalists.

THE following lectures have been given during the year at the University of Iowa under the auspices of the department of physics:

"The Ether," Professor K. E. Guthe, of the University of Michigan.

"The Measurement of the Charge of Thermions," Dr. J. C. Pomeroy, of Iowa State College.

"Some Physical Aspects of a Comet's Tail," Professor D. W. Morehouse, of Drake University.

"The Elementary Electric Charge" and "An Extension of the Brownian Movement Theory," Professor R. A. Millikan, of the University of Chicago.

"Applications of Least Squares in Physical Research," Professor Leroy D. Weld, of Coe College.

"Carriers of Positive and Negative Electricity," "Properties of the Wehnelt Cathode Rays" and "The Beaded Character of the Cathode Ray Line," Professor C. T. Knipp, of the University of Illinois.

THE subject selected for the Adams prize in 1914 is "The Phenomena of the Disturbed Motion of Fluids, including the Resistances encountered by Bodies moving through them." A theoretical rediscussion of the problem of fluid resistance may be undertaken, either in general or in simple cases, in the light of the experimental knowledge regarding the resistances and the nature of the broken motion of the fluid which is becoming available in the publications of the aeronautical laboratories of various countries. Information has been accumulating regarding the nature and mode of travel of meteorological atmospheric disturbances, such as cyclonic movements and line squalls, the propagation of minute waves of barometric pressure, and the nature of the lower boundary of the upper calm region of the air. A dynamical discussion of these topics, or of simpler problems in illustration of them, might be undertaken. The prize is open to the competition of all persons who

have at any time been admitted to a degree in the University of Cambridge. The value of the prize is about £220. The essays must be sent to the vice-chancellor on or before the last day of December, 1914.

UNIVERSITY AND EDUCATIONAL NEWS

In memory of a husband who for years had suffered from a malady that eluded medical skill, Mrs. George William Hooper, of San Francisco, has transferred to the University of California $1,000,000 for the establishment of an institute of medical research. The foundation is to be controlled by an advisory board of seven members constituted as follows: The president of the Carnegie Foundation, who is now Dr. Pritchett; the professor of pathology at the Johns Hopkins University, the director of the Rockefeller Institute for Medical Research, the president of the University of California, the dean of the Medical School of the University of California, E. D. Connolly, representing Mrs. Hooper, and a seventh member to be chosen by the western members of the advisory board.

The late Dr. Louis A. Duhring, formerly professor in the University of Pennsylvania, in his will disposes of an estate valued at about $500,000. His notes on medical cases are given to the university, and the will creates a trust fund of $25,000, the income of which is to be used for the benefit of the department of cutaneous medicine. The will gives the University of Pennsylvania Hospital $50,000 for the establishment of free beds in which cutaneous, cancerous and allied diseases shall be treated. After making a number of private bequests, the testator directs that the residue of the estate be given to the trustees of the University of Pennsylvania, and that it be applied to the treatment of cutaneous diseases and their study.

Mr. Stevens HeckshER has given $10,000 to the University of Pennsylvania to establish a fellowship in medical research.

Friends of Professor William Otis Crosby have presented to Columbia University the sum of $1,800 for the establishment of a collection of lantern slides to be known as the "William Otis Crosby Collection of Geological Lantern Slides."

The trustees of the University of Illinois have voted that for students entering in September, 1913, the requirements for admission to the College of Medicine (formerly the College of Physicians and Surgeons of Chicago) be raised to at least one year of collegiate work in addition to fifteen units of common and high school work, and that for students entering September, 1914, the minimum requirement further be increased to two years of collegiate work in some college or university of recognized standing.

Mr. C. L. Dake has been appointed assistant professor of geology and mineralogy in the Missouri School of Mines. He was instructor in geology at the University of Wisconsin during 1911–12, and during 1912–13 at Williams College.

Dr. Leo F. Guttmann, formerly head of the division of physical chemistry at the College of the City of New York, and for the last four years assistant professor of physical and industrial chemistry at Queen's University, Kingston, has been appointed associate professor of chemical engineering.

Dr. George Shannon Forbes, instructor in Harvard University, has been promoted to be assistant professor of chemistry.

Professor Oswald Külpe succeeds Professor Th. Lipps as professor of philosophy at Munich.

DISCUSSION AND CORRESPONDENCE

THE LAWS OF NOMENCLATURE IN PALEONTOLOGY

To the Editor of Science: A number of recent letters in Science on the subject of nomenclature may serve as an excuse to present to those interested a few of the special difficulties that beset the vertebrate paleontologist in questions of nomenclature.

The writer holds no brief for the law of priority. Names, scientific or popular, are, after all, but words designed to convey a certain concept, and the fixity and uniformity of that concept might quite well have been—or be—secured by an official dictionary which

should do for scientific names what the standard dictionaries do for words in general, namely, embody and fix as accurately as possible the current usage and significance of the word. That is what the proposal for an official list of scientific names amounts to, if carried out to the limit of its apparent trend. It would no doubt substitute references to types or descriptions for the dictionary definitions of meanings; but that is unessential.

The real objections to such a plan, as it seems to me, are (1) that the law of priority is so thoroughly imbedded in the mind of most systematists, and regarded so much as a moral or legal issue, a matter of justice to the first describer or of correct interpretation of certain statutory rules, rather than as a matter of convenience, that the authorities in systematic work would not abide by dictionary usage in the matter. (2) That any extensive list that could be prepared would certainly contain many names that were open to exception because the references or types so standardized were inadequate, or the current usage not approximately universal.

The first objection is illustrated by Dr. Dall's contemptuous rejection of the proposition that proposals approved by the majority of the committee on nomenclature should be submitted for endorsement or rejection to the body of the Zoological Congress. He will not abide by majority rule in the matter, even a majority of a committee of experts; and for a majority decision of "five dollar subscribers" he has no respect at all. The second objection is one that would be of special weight in any attempt to standardize the nomenclature of vertebrate paleontology.

But it has been said: If the systematists will not conform, let them go their way, and the rest of us go ours. To such a remark one can only say: Try to put such a scheme in the form of a definite program and see where it would land you. The scientific body is an organic whole interacting in all its parts, and Æsop's fable of the belly and the members is very much apropos. Altogether it would seem that the present methods and usages, annoying and exasperating as they often are to the teacher and morphologist, wasteful and time-consuming to the systematist, can not be modified to any material extent without causing further confusion.

The systematists are in the habit of assuring us that this confusion is only temporary; that when the laws of priority have been correctly and exactly applied to all species and genera, a stable and unchanging nomenclature will result; there will be no further changes. So far as vertebrate paleontology is concerned, I am certain that this optimism is unjustified and I doubt whether it is so in other branches of zoology. After the nomenclature has been revised it will be stable until somebody revises it again, just so long and no longer. Every new reviser, having new evidence at hand, or stressing differently the data already considered, is liable to interpret the case differently, and each difference in the interpretation of some obscure or minor point is liable to result in a whole series of alterations of well-known and important names. Only by forbidding the re-investigation of cases already authoritatively considered can changes be prevented. And that is just what Professor Ward's committee wants to do, and Dr. Dall makes it clear that systematists of the highest standing would not accept any such ruling. The plain fact of the case is that scientific nomenclature has come to a pass where the common name of a species is the only name with any permanency or prospects of permanency, and it is necessary to use it or to provide one if there is none already, in order that one's readers—aye, even other systematists—shall know what animal is under discussion. A century ago scientific writers wrote descriptions in dog-latin and then explained in good English or other modern languages what they were talking about. To-day they write descriptions under a scientific name dug out of some old forgotten treatise, and provide it with a wealth of learned synonymy, and then explain by means of the "dear old familiar name of the text-books" or the still dearer vernacular name, what animal it is that they are describing. Fashions change; not always in the way of progress.

In his last letter, Dr. Dall suggests a method of reconciling the differences between teachers and systematists by allowing the use of the "text-book name" with the status of a vernacular name, and a plus sign before it. I adopted a somewhat similar compromise some years ago, in a check list of American Tertiary mammals, only I put the commonly accepted name first, and the "correct" name afterwards, enclosed in brackets and with an equality sign before it. Now doubtless there are specific differences between this and Dr. Dall's discovery, but I claim that the *genus* is the same, and that therefore, *I* am entitled according to the law of priority of which he is so able a defender, to that statue which he expects to receive from the grateful teachers. Especially as I am sure my modification would be more acceptable to them, and while I feel less certain of his cordial approval, I don't see what legitimate exception he can take to it.

The vertebrate paleontologist is in some respects almost free from the difficulties in interpreting and applying the laws of nomenclature that beset his zoological brother. The literature with which he deals is mostly of recent date, and reasonably cognizant of the laws and decencies of nomenclature. There are only a few cases in vertebrate paleontology where there is any particular difficulty in fixing the type of a genus, the date of its publication or the species intended to be included under it.

His serious problem lies in the nomenclature of species, the identification of type specimens, and especially to know what to do with the fragmentary and almost indeterminate types of most of the older and many of the newer species, in relation to more complete specimens subsequently obtained. A quotation from Professor Marsh may be apropos.

> A single tooth or vertebra may be the first specimen brought to light in a new region and thus become the sole representative of a supposed new form. The next explorer may find more perfect fragments of the same or similar forms, and add new names to the category. A third investigator with better opportunities and more knowledge may perhaps secure entire skulls or even skeletons from the same horizon, and thus lay a sure foundation for a knowledge of the fauna.[1]

The wording is curiously suggestive of Professor Marsh's probable opinion of the activities of Leidy, Cope and himself in the field of American paleontology; but it is at all events a sufficiently accurate description of the general progress of the science. The earliest finds in any newly explored formation are generally fragments. They are new, they are of scientific importance, they are distinct from forms hitherto known, they ought to be described and figured, and they ought to be named as a matter of convenience in scientific discussion. But they will undoubtedly make trouble later for the systematist. The "next explorer" must either "add new names to the category" or identify one or more of his fragments with the first described type. And if his material comes from a different locality such identifications may cause serious errors in stratigraphic correlation. The third investigator may ignore the earlier types as too incomplete for identification, or he may arbitrarily identify them with such of the species secured by him as suits his convenience. Either method will subject him to criticism and be liable to mar the scientific results of his investigations.

It is a covenant universally accepted that a new species is not to be described unless it can be shown or inferred to be different from all previously described species. But here it simply can not be applied. The third investigator may have at hand skulls and skeletons of a dozen species all clearly distinct from one another, yet any one of them may be cospecific with the tooth or fragment on which an earlier species was founded, and it is often absolutely impossible to find in the type any characters that are really valid evidence for referring to it one rather than another of these later discovered species.

The difficulty in treating of these more or less indeterminate species recurs again and again in the literature of vertebrate paleontology, causing endless confusion and error when arbitrary identifications are subsequently

[1] "The Value of Type Specimens," *Am. Jour. Sci.*, 1898, Vol. VI., p. 402.

found erroneous and infinite recrimination and heartburning when the work of earlier authors is set aside or ignored. These troubles we shall have with us always; but perhaps their amount might be reduced if an intermediate course were adopted.

The earlier type may be a specimen showing unmistakable ordinal, family or generic characters, but not adequate as a specific type. Let it stand so. Do not set it aside as "indeterminate," but specify the extent to which it *is* determinable. It can remain in the literature and be included if desirable in faunal lists, but additional material should not be referred to it unless the new specimens be topotypes, *i. e.,* from the same locality and the same geological level, so far as these are recorded or can be safely inferred from the literature, unpublished notes or labels or the appearance of the specimen. If it has valid generic characters a genus founded upon it is valid, and other species may be referred to it; if it has family characters but no distinctive generic characters, a family name founded on the genus is valid, but no subsequent genera are to be synonymized with it except when species of those genera are known to occur in the locality and geological horizon of the older genotype species. In illustration a few cases may be cited:

1. *Anchippodus riparius* Leidy 1868, type a lower molar from the "Miocene" (? Oligocene) of New Jersey. Type of the family Anchippodontidæ Gill 1872, referred to the order Tillodontia Marsh 1875. Leidy referred to this genus and species in 1873, his *Trogosus castoridens* 1871 based on a lower jaw from the Middle Eocene (Bridger formation) of Wyoming and to the same genus Marsh's *Palæosyops minor* 1871, based on a lower molar. Marsh, subsequently obtaining complete skulls and skeletons of related animals, accepted Leidy's genus *Anchippodus,* described a new genus *Tillotherium* with three new species, and based upon it the family Tillotheridæ which he made typical of the order Tillodontia.

No topotypes of *Anchippodus riparius* are known. Subsequent authors have either followed Marsh in ignoring Gill's name, while accepting Leidy's identification of *Anchippodus* with *Trogosus,* and considering *Tillotherium* as distinct from the latter, or they have used Anchippodontidæ as the family name, while deriving all the characters of the group from Bridger materials.

The result is that the faunal lists record in the New Jersey "Miocene" along with a known Oligocene mammalian fauna (*Cænopus, Entelodon, Protapirus*) a genus and species of the Middle Eocene fauna, while the western collections make it reasonably certain that in those regions the family and order disappeared with the Middle Eocene. Were this conclusion supported by real evidence, it would lead to some interesting corollaries as to migration and survival. In fact it is quite misleading. The type of *Anchippodus riparius* is inadequate for specific or generic comparison, and doubtfully adequate for family or ordinal comparison. It is very improbable that it is congeneric with *Trogosus,* hardly possible that it is co-specific with *T. castoridens,* so far as one may judge from the associated fauna in absence of generic or specific characters in the type specimen. Gill's family characters were drawn from *Trogosus,* and since it is doubtful whether this genus belongs in the same family with *Anchippodus,* his family should be held as doubtfully synonymous with Tillotheriidæ, both names to be retained, but the former as "? Anchippodontidæ Gill, fam. indet."

2. *Hippodon* Leidy is the first genus of three-toed horses described from this country. The type is *H. speciosus,* based upon a lower molar tooth. Leidy subsequently referred to the species upper teeth, etc., which he considered congeneric with the older European genus *Hipparion.* On the basis of these and other referred specimens the species was held valid and the genus a synonym of *Hipparion* until Gidley revised the three-toed horses in 1907. No topotypes were or are known. Gidley set aside both genus and species as indeterminate. I subsequently identified and located the type specimen which had been missing, and after making a fairly careful com-

parison came to the conclusion that it was a species of *Merychippus*. A more thorough restudy of the Miocene horses last summer brought me to the conclusion that this tooth, while certainly distinct from *Hipparion*, lies somewhere near the border line between *Merychippus* and *Protohippus*, but on which side of the line I can not determine except arbitrarily. The species is, therefore, in fact indeterminate generically, and a valid genus can not be based upon it. *Hippodon* would, however, stand as the type of a group including *Merychippus*, *Protohippus* and *Pliohippus* as contrasted with *Hipparion* and *Neohipparion*. In stratigraphic correlation of the beds at Bijou Hill, where it was found, it would be listed under the Protohippinæ as *Hippodon speciosus* gen. et sp. indet.

3. *Deinodon* Leidy is determinable as to family, but is not determinable generically, as the genera of carnivorous dinosaurs are now distinguished. The same is true of a whole series of genera and species described by Leidy and Cope from the Judith River. The treatment of types and referred specimens of these genera by paleontologists as specifically distinguishable or identical has sadly misled Dr. Peale in his recent discussion of the vertebrate evidence as to the age of the Judith River beds, leading him to present as conclusive evidence of identity in age a correspondence in fauna which to those who know the nature of the specimens on which the lists are based is no evidence at all.

In brief the plea is for the full recognition of nomenclature laws, but for the avoidance of arbitrary or unprovable identifications in the future, and the recognition of the actual facts as to the extent to which described genera and species are truly determinable. The allowed exception in the case of topotypes is based upon an inference of identity which it would seem impossible ever to prove incorrect. In all other cases the chances that future discovery may upset an arbitrary identification should prevent its being used as a basis for changes in nomenclature.

The source of the present lamentable situation in nomenclature is that an excellent system of procedure, designed to settle unsettled questions, has been wrenched from its intent and used to unsettle settled questions. The present writer, having studied with more or less care the majority of the type specimens of American fossil mammals and reptiles, has abundant evidence at his command to upset by a strict application of the accepted laws and procedures, much of the present nomenclature, including many of the alterations proposed in recent years upon grounds of priority. But he has no intention of so misusing his opportunities, or of being responsible for such changes until convinced that they will really result in greater stability.

W. D. MATTHEW

HOW IS THE WORD FOOD TO BE DEFINED?

THE query expressed in the title "How is the word 'food' to be defined?" is suggested by a restrictive usage of this word which is rather prevalent in American text-books of elementary botany, and which seems to have originated among American plant physiologists. Presumably it had its birth in university courses in botany where the arguments for its use were given and understood, but as it appears in the elementary texts, it involves a marked inconsistency of thought and expression for which no provision is made. Since it represents a striking divergence from the ordinary meaning of the term "food," it deserves wider consideration, looking either toward its general adoption, if desirable, or else toward its discontinuance.

The word food, according to its ordinary connotation, is applied to any substance which, when taken into the body of an organism, can be used by that organism in the construction of new tissue. Definitions of essentially this content are to be found in the Century, Standard and Webster dictionaries. Using this definition as a basis, we should consider as food for green plants the water, carbon dioxide and mineral salts absorbed from the surroundings. According, however, to the restricted usage, these are not considered as "foods," but are referred to as "raw materials," "nutrients," "food materials," or some other cir-

cumlocution. Bergen and Davis[1] have a sentence which shows clearly how the restricted usage conflicts with the general usage illustrated by the definition given above:

> The series of processes by which the plant (1) takes in *raw material* to form its *foods*, (2) unites these into *foods*, and finally (3) constructs tissue from these *foods* or (4) stores them, constitutes nutrition.

Gager in a recent book-review[2] has given another excellent illustration of the same conflict of usages in the sentence which follows:

> On page 38, mineral *nutrients* are erroneously called plant *food*. [The italics of both quotations are mine.]

Judging from these quotations, it is evident that the content of the newer usage is entirely different from the older general usage. Carbon dioxide, water and mineral salts, all clearly to be classed as plant food under the older definitions, can not be so classed according to the newer usage. By a process of exclusion, after a consideration of the quotations just given, we arrive at the following new definition of the word food, viz., *organic materials available for immediate assimilation*. It appears, however, from other discussions that the intentions of the proponents is to apply the term food also to the organic raw material used by animals or colorless plants.

Two questions arise from the foregoing consideration: (1) Why has the new meaning of "food" arisen? (2) Does it deserve to prevail?

The arguments for the restricted usage are derived mainly from a comparison of the nutrition of green plants with that of animals. The food of an animal is chemically practically the same material as the tissues of the animal and consists of proteins, oils, fats and carbohydrates. (Mineral matter may be excluded from the consideration for the present.) During the process of digestion, this food is temporarily simplified as far as may be necessary to make it soluble. Assimilation consists merely in the reconstruction of compounds in general like those found in the original food. In the case of green plants, all the materials obtained from the surroundings are simple inorganic substances. The process of preparing them for assimilation is a complex synthesis, carried on by means of energy derived from an external source. At the end of this process we find ready for assimilation substances of the same sort as those which result from animal digestion. The ensuing process of assimilation is the same in green plants as in animals. These differences and similarities in materials and processes form the basis for the revised definition of the word food.

The reasons for adopting the new definition have been discussed in detail by Barnes.[3] They may be briefly recapitulated as follows: Protoplasm, being the same in green plants as in animals and colorless plants, and the material which it can actually assimilate being always organic, it creates an undesirable antithesis in thought to recognize as food for living things both inorganic and organic substances. Carbon dioxide and water if recognized as food for green plants can be so considered only for the chlorophyl-bearing cells, and for these only in the presence of light. They can not be used as food by the chlorophyl-less cells at any time, or for green cells in the absence of light.

Notwithstanding the weight and authority of the arguments in favor of restricting the meaning of the word food, there are numerous objections which should be given consideration.

One of the principal objections to be noted arises from the fact that the difficulty noted by Barnes and others is mainly of academic interest. So far as I have been able to discover, the question has been discussed only in two treatises of plant physiology designed for use by university students (Barnes and Green). Apparently, then, to be thoroughly conversant with the new usage, it is necessary

[1] "Principles of Botany," p. 106, 1906.

[2] Payne's "Manual of Experimental Botany," *Torreya*, 12: 134.

[3] Coulter, Barnes and Cowles, "Physiology," 3: 356–8.

to have used one of these texts or to have pursued an equivalent course in plant physiology.

Correlated with the objection just noted is another concerned mainly with the teaching of the restricted usage in courses in elementary botany in secondary schools. The clearest approach in beginning a course in botany in a high school lies in leading the pupil to think of plants as separate living things, each of which is an individual, which has, like an animal, its problems of food getting, nourishment, protection, etc. The university concept of the word food, however, requires that the pupil think of a green plant as an aggregate of different kinds of cells which bears a very different relation to its surroundings as regards its food than the living things, *i. e.*, animals, with which the pupil is familiar. The pupil thus loses the definiteness of the idea of a green plant as an individual with problems like those of animals, and has to think of it as something which does not get its food from without, but must manufacture it within its cells. The phraseology of this usage of the word food has been written into the elementary texts without, so far as I have been able to find, any attempt to make the pupil understand how or why it differs from the older usage. As a consequence he learns to use the word food, in the class at least, in a very different way from his ordinary understanding of it, but usually without any realization of the inconsistency.

Complications follow the restriction in meaning which do not appear to have been realized. In the case of green plants food, in the restricted sense, includes only organic material prepared within the cells of the plant and available for assimilation by any of the cells. In the case of animals, food is first, the organic material which, if taken into the alimentary tract, is able to be digested, and second, the material resulting from such digestion, even yet extra-cellular, but comparable with the material recognized as "food" of green plants. Thus it appears that future dictionaries will need to give at least two definitions of the word food.

If we accept the modified definition of food as desirable, we shall then have to face the task of making it part of the common knowledge of all who use the English language. Under present conditions it is practicable to teach it only to the minute proportion who pursue courses in plant physiology in colleges.

Referring to the antithesis in thought to which Barnes objected, it may be noted that this has apparently given little or no trouble to a number of well-known botanists who have discussed plant nutrition in text-books. It appears to have occasioned no difficulty in the elementary texts of Atkinson and McDougal; in the general texts of Bessey, Sachs, Strasburger, etc.; in the physiological treatise of Jost. Ganong in his text-book of physiology refers to the restricted meaning as desirable but as probably impossible to promulgate.

It seems to the writer as entirely unnecessary to attempt to make so great a distinction between the food material of the individual green plant as a whole and the food material of its constituent cells, or between the crude food materials of green plants and animals. It is possible sufficiently to differentiate the materials and their processes of preparation without revising out of conscience the ordinary meaning of an old and useful word. It would appear sufficient to satisfy all the needs of discrimination to use expressions like "crude food" and "cell food." [Since the preceding sentence was written, practically the usage suggested there has been used in high school classes with good results. The use of the expression "cell food" emphasizes to pupils the idea of the cells as the unit of structure and function in living things.]

Finally it may be noted that in the last analysis, it is strictly impossible to restrict the word food wholly to organic material. Barnes limited his discussion of the question to carbon dioxide and water and the carbon compounds resulting therefrom. He expressly excludes mineral salts from his consideration as too small in amount to deserve attention. Logically, however, they can not be excluded even on this basis and especially not in view of the fact that the nitrogen, sulphur and phosphorus of protoplasm are derived from

mineral matter. Moreover, although it is relatively easy to distinguish between CO_2 and H_2O on the one hand as inorganic "food materials," and sugars, starches, etc., on the other hand as manufactured "foods," who can say when nitrogen, sulphur and phosphorus cease to be "food materials" and become "foods"? Is it not more than probable, also, that some constituents of the mineral material taken in by plants and animals are immediately available for assimilation in the form absorbed, and are thus foods in both the restricted and broader senses of the word? If the facts are as here suggested, it is clearly impossible to limit the term food to organic material, first because too little is known of the metabolic processes by which nitrogen, sulphur, phosphorus, et al., are assimilated to enable any one to say at what stage these elements cease to be parts of inorganic and become parts of organic compounds, and second, because some inorganic substances are probably *foods* in both senses of the word.

In conclusion, the question asked in the title may be repeated. How is the word food to be defined? Is it to be limited to organic substances with all the pedagogic and scientific difficulties which such limitation entails? Or shall it remain as at present, raising no practical difficulties whatever and leaving the academic difficulties involved to be dealt with when the pupil becomes sufficiently mature to understand them?

Ralph C. Benedict
High School of Commerce,
New York City

A STANDARD FORM OF COMMITTEE MEETINGS

Distances in the United States are so great that it is often impossible for a committee to hold a meeting, and its work must be done by correspondence. Owing to the international, or national, character of many committees, and the increasing amount of friendly cooperation among scientific men, some standard system of arriving at results is greatly needed. It would then only be necessary when appointing a committee to state that its work would be done in this way, and the chairman would be saved the necessity of devising a method in each case, and the doubt in many cases, whether he was justified in appending the names of all members to his report.

The following method is accordingly suggested: The chairman or secretary should have three letters manifolded, and sent in due course to each member of the committee. The first of these should state the exact terms of the appointment; the objects desired; a request for suggestions for the report; an opinion whether a meeting is advisable, and if so, when and where.

The second letter should contain a preliminary report embodying the suggestions received, and in cases of doubt asking numbered questions to which, if possible, the answer will be yes, doubtful, disapprove, or no. In the first three cases, the writer accepts the views of the majority of the committee. In all four cases, he authorizes his name to be attached to the report, provided that it contains a statement that he dissents from the questions to which his reply is no. Prompt answers are requested, but if any member fails to reply after a letter has been in his hands for a week, the chairman may assume that he assents.

The third letter should contain the proposed report, to which all the names would be attached unless answers were received expressing dissent. Some of the members might prefer to make a minority report. If no reply was received to letters one and two, letter three should be registered with a request for a receipt, as otherwise the previous letters might not have been received. If haste is important, night letters are generally to be preferred to telegrams, since the delay from the most distant points of the country would seldom exceed twenty-four hours. If a reply by cable is necessary, the chairman should give his cable address, and if possible arrange all his questions so that each answer shall consist of only one or two words. A reply by cable, in which the fifth question related to a place of meeting, might read: Fieldsmith, Washington. One, two, yes; three, no; four, doubtful; five, London, July. Brown.

Edward C. Pickering

May 6, 1913

QUOTATIONS

UNIVERSITY LIFE IN KANSAS

The vague feeling of unrest that has prevailed among the members of the university faculty on account of sweeping changes that might be made by the new board of administration has deepened into real alarm with the announcement from the board that all positions at the university have been declared open.

This is understood to mean that when the new board takes charge formally July 1, the entire faculty must be reengaged. The fact that a member has been elected a permanent member of the faculty by the board of regents in previous years after serving an apprentice term of years would not necessarily count at all with the new board. The board has by its announcement indicated that it will feel free to drop any member of the faculty it pleases.

That such will be the attitude of the new board is indicated by its action at a meeting last week when Chancellor Strong and President Waters were "reelected" to the positions they now occupy. As there was no definite limit to their "terms" it is hard to explain the action of the board other than by the supposition that it is its intention to wipe the slate clean and build the university anew "from the ground up." If any person has a position on the faculty after July 1, he will hold it directly from the new board of administration and not by virtue of the fact that he has grown old in the service of the institution.

Naturally, this plan of procedure has made the faculty very uneasy. It has been customary to reelect a new member for a number of years, until he had proved his worth to the institution, and then the regents would elect him a "permanent member" of the faculty. Under the new rule the old members are placed in the same boat with instructors of a single year's standing. None of them will know until after the "election" whether they are to be turned on the faculty. It is not an uncommon thing for a professor to have enemies. How is he to know that his enemies may not have the ear of the board, spreading little stories that reflect upon him? The old board of regents made it a practise to pay very little attention to such stories, although they heard plenty of them. But what the new board will do is entirely a matter of conjecture.

The most envied members of the faculty at the present time are those who have had offers of situations elsewhere. Of course everything may turn out all right, but, on the other hand, men who otherwise would have nothing to be alarmed at may be eliminated when the new broom begins to give its exhibition of clean sweeping. And a good many teachers have remarked privately in the last few months that they intended to take the first fair offer from elsewhere that presented itself. The talk about abolishing and consolidating and transferring courses was enough to make them uneasy, but the announcement now made that every teacher is likely to be treated as if he were for the first time an applicant for a position at the university has caused a decided feeling of insecurity.—*Lawrence Gazette.*

SCIENTIFIC BOOKS

A History of European Thought in the Nineteenth Century. By John Theodore Merz. Vol. III. (Part II., Philosophical Thought). New York, Charles Scribner's Sons. 1912. Pp. xiii + 646.

Nothing so well illustrates the profound interest of the great subject undertaken by Dr. Merz as the contrast between his work and Whewell's "History of the Inductive Sciences" (3d ed., New York, 1858). The evident superiority of the later history, especially in intensive treatment and exact *Facharbeit*, is in itself an index of the wonderful progress that characterized the nineteenth century, notably after "The Origin of Species." Fortunately, too, Dr. Merz has been content to take time. His first volume was published in 1896 (3d ed., 1907), and in it he grappled with the physical sciences. The second volume followed at an interval of seven years, and completed the task, as concerned the "sciences of nature." These volumes should be in the hands of every builder of "natural knowledge." It is to be hoped that the appearance of the

volume now before us will lend new stimulus to the sale of its predecessors. A fourth and concluding volume is yet to follow. But we have evidence, and to spare, that, once more, an Englishman of business affairs has arisen to occupy a place alongside George Grote and many others (Darwin not least) who, without academic support or connections, have accomplished so much for the advancement of British science, and the preservation of its distinctive temperament. As it stands, "A History of European Thought in the Nineteenth Century" is a magnificent performance. To the patient thoroughness of the German, Dr. Merz has added the clearness and, in the best meaning, the common-sense of his own countrymen. He carries his load without the aid of any partisan theory, he has no pet ideas to exploit. And although, the very nature of the case preventing, I am not yet convinced that his discussion of philosophical thought is as successful as his presentation of scientific achievement, nevertheless it is plain that Part II. bids fair to be as invaluable as Part I.

The volume contains six chapters: Introductory; On the Growth and Diffusion of the Critical Spirit; Of the Soul; Of Knowledge; Of Reality; and Of Nature. The plan demands some elucidation. Dr. Merz points out that "In the beginning of the century, both in Germany and England, science and scientific thought played only a secondary part in literature and teaching. France was the only country in which it had early acquired that position and commanded that esteem which it now enjoys everywhere" (p. 91). The nineteenth century brought about a change which "amounts in many cases to a complete reversal of the estimation in which the mathematical and natural sciences, on the one hand, the historical and philosophical, on the other, are held. The earlier part of this history has furnished the answer to the first half of the problem: I there endeavored to show that the success and assurance of scientific thought has grown with the growth and diffusion of the scientific spirit, which has been more clearly defined as the exact or mathematical spirit. . . . The second part of this history will have to answer the other half of the above question, namely, what are the causes that have brought about that great change in the general and popular appreciation of philosophical discussions? How is it that instead of one or two dominant systems of thought we have now what may be called a complete anarchy, or, at best, a bewildering electicism? . . . I will at once answer this question. The great change referred to is owing to the growth and diffusion of the critical spirit, taking this term in its widest sense" (pp. 93–95). Accordingly, the scene being thus shifted, there is no little need of the warning that Dr. Merz is careful to issue. "I think it will be more helpful to my readers if, when entering on a new portion of my subject, I immediately impress upon them the necessity of adopting an entirely different point of view from that to which they may have become accustomed by the perusal of the former volumes. So strongly do I feel the necessity of this, that I am inclined to say that, except they are prepared to familiarize themselves with an entirely altered set of interests, problems and methods, I shall fail to gain, or to retain, their attention in that which follows" (p. 34). This, then, indicates the general setting.

The method employed to reach the special divisions is also set forth clearly. "The dualism which pervades all modern thought will occupy us quite as much as the attempts towards unification" (p. 56). "In Leibniz philosophical thought arrived at the position which, with certain interruptions, it still occupies at the present day; its task being, not to afford new knowledge, but to mediate between the claims of two kinds of knowledge: that which deals with things surrounding us in time and space, and that which deals with the highest questions of our life, our destiny, and our duties" (pp. 334–335). Thus, following Kant's famous pronouncement, at the close of the "Critique of Practical Reason," Dr. Merz finds himself confronted with two central problems. "The first begins with the place which I occupy in the outer world of the senses and expands the connections in which I stand into the invisibly great, with worlds upon worlds

and systems upon systems, moreover, into limitless ages of their periodic motion, its origin and duration. The second begins with my invisible Self, my personality, and represents me as standing in a world which has true Infinity, but is accessible only to Reason." Each of these problems, in turn, splits into several parts. Therefore, Dr. Merz proclaims: "I am not primarily interested in expounding the different philosophical systems, but rather in tracing the leading ideas which have survived these systems themselves and become the common property of the philosophical mind at the present day. . . . As we saw that the scientific activity of the century resulted in the firm establishment of a small number of leading conceptions, so I shall now endeavor to show how the huge and frequently conflicting philosophical literature has left behind it a small body of guiding ideas which form the enduring bequest of nineteenth-century speculation" (pp. 39–41). Hence, "looking at the different national interests which promoted philosophical thought in the three countries [*i. e.*, England, Germany and France], we are led to a first division of this great subject which is given by the terms psychological, metaphysical and positive" (pp. 45–46). Besides this, there is the sphere of individual beliefs and convictions which "have quite as much the right to be regarded as facts as any more definite, scientific or historical knowledge" (p. 53). The latter are to be treated in Vol. IV., and again in six chapters: Of the Beautiful; Of the Good; Of the Spirit; Of Society; Of Systems of Philosophy; and "will close with a summary of the general outcome of Philosophical Thought during the Nineteenth Century" (p. vi). Of the former, now before us, but one seems to call for further comment, the rest are self-explanatory. In these days, what does Dr. Merz mean by "Of the Soul"? He answers: "I have headed this first chapter which deals with a definite philosophical problem: 'Of the Soul.' I might have chosen several other words which would have equally introduced us into that portion of philosophical literature with which I am now concerned. . . . That I nevertheless prefer to speak of the soul and not of the human mind or human nature, may be justified by the fact that the word soul introduces us at once to a historical discussion which took place in the middle of the century in Germany, and which may be considered to mark one of the great changes that have come over our way of regarding all questions connected with the mental life. What was called at the time 'Die Seelenfrage' occupied the foremost place in philosophical discussions carried on both by philosophers and naturalists. . . . It seems appropriate to start the history of philosophical thought with an account of the problems which center in the word soul" (pp. 196–199).

Thanks to limits of space and to the fact that technical criticism of philosophy is out of place here, I must content myself with a few summary remarks about the contents of the book. The introductory chapter offers an admirable review of the temperamental differences between science and philosophy, and of the conditions that governed reflective thought throughout last century; while the chapter on the critical spirit is the best synopsis of the historical sciences within my knowledge. Seeing that the scientific and critical movements are the twin intellectual achievements of modern thought, and that the one can not be understood apart from the other, this distilled statement should prove most illuminating to all workers in the physical and biological fields.

Turning to the philosophical chapters, the point of view may be hinted. It is sufficient to say, perhaps, that Dr. Merz deems Lotze the most typical and discriminating thinker of the age. Consequently, he tends to pivot German thought upon the Göttingen professor and, as a sequel, to lay much stress upon Renouvier for French and Professor James Ward for English philosophy. Seeing that Renouvier has not dominated French thought at any time, and that Dr. Ward has never wielded such influence in Britain as Green, the Cairds, Wallace and Bradley, this view seems difficult to maintain. Despite Lotze's failure to found a "school" in Germany, Dr.

Merz's contention may be justified from a purely historical standpoint, although, even here, I have grave doubts (*cf.* pp. 266 f.). For, the clear statement of Lotze's position (pp. 501 f.) amounts to a fatal criticism philosophically! The single proposition—"relations which endure and events that happen, imply things in and between which they subsist" (p. 502)—is in itself sufficient condemnation. Another interesting feature, interesting especially to scientific men, is the rehabilitation of Schelling (pp. 453 f.), who, we are told, "deserves to be looked upon as the central figure during the idealistic period of German philosophy" (p. 453). Now, although Dr. Merz seems to me to begrudge the immense influence of Hegel, he is bold enough to affirm that "Hegel deserves to be looked upon as the greatest representative of philosophical thought in the nineteenth century" (p. 476). Of this there can be no question, I think. But how this conclusion, which Hegel literally wrings, as it were, from Dr. Merz, is to be reconciled with the primacy accorded to Schelling is hard to understand. Nevertheless, the appreciation of Schelling, and particularly the effort to remove the misconception that has been heaped upon him, was greatly in need. Omitting many other notable matters, I would simply record that the chapter on knowledge is, in my judgment, the most successful; while the discussion of the problem of nature is the most suggestive, so much so, that it can not fail to appeal to followers of the natural sciences. Dr. Merz handles the vast wealth of material with astonishing skill, intimacy and perspicacity.

As was inevitable in work done on so large a scale, there are some few unguarded statements. I can not but think that the tendency to separate sharply between "outer" and "inner" results in a false contrast (p. 12). It would be much nearer the truth to say of D. F. Strauss, that the issues he raised were misunderstood by his own contemporaries, than that "the conclusions he came to were premature" (p. 169). It is doubtful, if no more, whether any such relation between Hume and Kant as is put forward for fact (p. 229) could be proven historically. The remark about psychology (p. 252) is scarcely in focus. For, even granted that the old psychology disappeared, we had ample compensation *in Völkerpsychologie* and *Sprachwissenschaft,* both traceable to the very movement which Dr. Merz tends to condemn. Indeed, the main defect of the "History" is to be found precisely in its prevalent tendency to minimize this same movement. There is an astonishing misconception of Fichte's problem (p. 234), and a curious comment about Spencer's knowledge of Kant (p. 296), whom, as Spencer himself informs us, he could not read. It ought to be noted, finally, that the scope of the work is not European. Dr. Merz really confines himself to the three leading nations—France, Britain and Germany. The omission of Italy, particularly after the work accomplished by her when her political unification was won, is to be regretted. But, we should not look a gift horse in the mouth. These are mere blemishes, never blots, on a very remarkable achievement.

The publishers (Blackwoods, Edinburgh) ought to have their share of commendation. Considering the size of the volume, and the elaborate notes with which it literally swarms, the press work is exceptionally free from errors. A letter dropped in the marginal summary (p. 12); J. F. for J. H. Tufts (p. 57 n.); J. M. for T. M. Lindsay (p. 209 n.); Taylor for Tayler (p. 306 n.); Eucken's work (p. 436 n.) is not a "little tract"; Thompson for Thomson (p. 612 n.)—an insignificant total. The index is excellent—a most important consideration in so voluminous a performance. Dr. Merz promises that, "when the fourth volume appears," it "will be cancelled to make place for a more comprehensive index covering both volumes" (p. vi). In these circumstances, I venture to append a list of errors for correction then. Under De Morgan, "Study of . . . Metaphysics, 576" should read Study of . . . Mathematics, 376; "M'Cormick" should read McCormack; there is a reference (p. 165 n.) omitted under "Lexis." "Ravaisson-Mollien" is misleading. Ravaisson did adopt the name of his maternal

uncle, but not till after his classical "Rapport" (of 1867, not 1868, as on pp. 201, 234, 426); quite rightly, he is always referred to in the text by his paternal name, without the later addition, and should be so noted in the index. Under "Schiller" philosophy has been substituted for history. "Ænesidemus" is correctly printed under "Schulze, G. E.," incorrectly on p. i of the index. Under "Ueberweg," T. M. should be substituted for J. M. Lindsay; and "Taylor" should read Tayler. The caption "Cause and effect defined" should be thoroughly revised and extended. There are several references in the text of far greater importance than the single one recorded in the index.

R. M. Wenley

Ann Arbor

Electrical Machine Design. By Alexander Gray. McGraw Hill Book Company.

"Electrical Machine Design," by Alexander Gray, discusses the theory of operation and design of direct current generators and motors of both interpole and non-interpole type, alternating current generators, induction motors and transformers. Five hundred and seven pages are not enough to cover such a range of subjects satisfactorily, and when the analysis of theory is carried to the extent attempted by Mr. Gray the result can not be a success. Considered as a text-book, it would be unsuited to the average fourth-year student, not because the analyses are too involved for such, but because their introductions are too brief. The calculation of temperature gradient may be taken as an example. If a few paragraphs had been inserted discussing the laws governing flow of heat, and containing perhaps a simple application, the subsequent treatment would have been much more easily understood. The same criticism is applicable to the chapter on armature reactions in alternators.

Considered as a book for reference purposes, this work contains much matter of value, both to the student and to the designer. Discussions of such questions as noise of induction motors, comparative value of shell and core type transformers, short pitch windings in direct current machines, are really valuable and are not to be found readily elsewhere. The subjects of commutation and insulation are very well developed.

The arrangement of subject matter is usually excellent. The treatment of the induction motor had better have followed that of the transformer instead of preceding it. Such an arrangement would have made possible the consideration of the induction motor as a transformer, a most practical and effective method. The theory of operation and construction of each type of apparatus is first developed. This is followed by the procedure in design, the discussion of special types of machines, and a chapter on specifications. Examples accompany the text and should aid the student materially in his comprehension of the subject.

The book compares favorably with the other books on design in our language, but when it is contrasted with the simple and extremely logical treatments to be found in the works of Arnold, its own shortcomings are most apparent.

C. W. Green

METEOROLOGICAL OBSERVATIONS AT THE UNIVERSITY OF CALIFORNIA

It is probably due to the fact that the public interest in meteorology is centered around weather forecasts that the science has received so little attention from the universities of the United States. The University of California is one of the relatively small number which has maintained a regular series of observations for a considerable period.

Until July 1, 1912, when the routine meteorological work at Berkeley was transferred to the department of geography under which the courses in meteorology and climatology are listed, the astronomy departments, the Lick Observatory at Mount Hamilton and the Students' Observatory at Berkeley, carried on the principal meteorological observations of the university. Meteorological observations have always been a part of the regular work of the Lick Observatory and, when the Students' Observatory was established at Berkeley, its ac-

tivities included meteorological work as a voluntary observer's station of the United States Signal Service. The first rain-gauge was set up on October 16, 1886, and the meteorological work may be dated from that time. A synopsis of the results of the observations for the twenty-five years ending July 1, 1912, has been prepared by the director of the observatory and is soon to be published in the University of California Publications in Geography, which will contain, among other geographic papers, the results of the meteorological observations and climatic studies made at the University of California.

There is, perhaps, no type of work in which so much depends upon the daily exacting attention to detail as meteorology. An observation missed in this work is lost forever; an approximate figure may be obtained and used in the preparation of the averages, but the greatest value of meteorological work depends upon an unbroken and regular series of observations. Such a series is that which was obtained by the Students' Observatory at Berkeley during a period of nearly twenty-six years. From the establishment of the meteorological station until September, 1892, the observations were made three times daily, at 7 A.M., 2 P.M. and 9 P.M., and the results sent to the signal service at the end of each month. From September 1, 1892, observations have been made at 8 A.M. and 8 P.M., Pacific Standard Time, the standard time in use in the state of California. The transfer of the meteorological work from the signal service of the War Department to the Weather Bureau of the United States Department of Agriculture in no way broke the continuity of the record at Berkeley; the relations which were established with the signal service have been continued with the Weather Bureau.

The plans of the signal service for the work of the voluntary observers proved to be somewhat more detailed than seemed desirable under the conditions of the service and in the early nineties a simplified form of report was adopted. As far as the reports to the Weather Bureau were concerned the University of California conformed to the new plan; but the observations at Berkeley were maintained on the same plan as before, as there was nothing inconsistent with this in the new form, with the result that the record kept by the university is exceptionally complete.

In addition to the reports which have been sent to the Weather Bureau and printed in its publications, the university bas issued a monthly meteorological synopsis of Berkeley regularly since the beginning of 1887. General synopses have been prepared by Professor Leuschner, the director of the Students' Observatory, and published at five-year intervals, which have summarized the results, as shown by the monthly synopses, up to the date of the general summary. The monthly synopses have been continued by the department of geography in a somewhat enlarged form. It is also proposed to publish an annual report in conformity with the suggestion of the International Meteorological Committee. The original record is in such good condition that, when it was decided to change the method of compiling and stating some of the data in order to bring the form of the synopsis into accord with the better practise, this could be easily accomplished without the use of approximations, and without breaking the series.

There is no scientific work which will continue itself for any considerable length of time without the persistent efforts of men. The meteorological work at Berkeley depends largely upon the efforts of the directors and the Students' Observatory. With the advice of the signal service and the approval of the University of California Professor Frank Soulé, the first director, conceived the idea of making meteorological observations at the observatory, and began the work which has continued since that time. In 1892 he was succeeded as director by Professor A. O. Leuschner, who has maintained the record at its high standard for the past twenty years. The actual observations have been made by various members of the staff of the observatory; and their faithful and punctual performance of an exacting and often unin-

teresting routine duty gives the record its value.

The meteorological work has now come under the direction of the writer as a part of the work of the department of geography. The observations are made at 8 A.M. and 8 P.M., 120th meridian time, besides which there are continuous records of pressure, temperature and relative humidity from the recording instruments. The eye observations are now as follows: wet and dry bulb thermometer readings, maximum and minimum temperatures for the preceding twelve hours, air pressure, wind direction and estimated velocity, amount of cloud, weather and precipitation during the preceding twelve hours. The data are summarized and published monthly in the *Meteorological Synopsis* of Berkeley and a monthly report is made to the United States Weather Bureau on the regular form of report for the cooperative observers.

The University of California has for Berkeley and for Mount Hamilton meteorological records of considerable length and more complete than exist for many places in the United States not regular stations of the Weather Bureau. In a state where climate is such an important factor in the life of the people as it is in California, it is proper that the educational institutions, but above all the state university, should pay more than ordinary attention to meteorology.

William G. Reed

Berkeley, Cal.,
March 1, 1913

REVIEW OF FOREST SERVICE INVESTIGATIONS[1]

The new periodical issued by the Forest Service, the *Review of Forest Service Investigations,* is the direct outcome of the standardization and coordination of the investigative work done by the service. This investigative work has been placed on a more solid footing by the establishment of investigative committees in each district and of a central investigative committee in Washington.

[1] Volume I., issued March 11, 1913, by U. S. Department of Agriculture, Forest Service.

The *Review* is to serve as a medium for keeping foresters in touch with the scientific work of the profession in America. It will do this by publishing progress reports on major investigations the completion of which will require a number of years, during which time nothing would otherwise be known of them; by publishing full reports on minor studies not of sufficient importance to warrant publication as separate bulletins or circulars, but which nevertheless contain valuable material; and by giving a general view of the scientific forest problems in this country and of what is being done toward their solution. In short, the district and central investigative committees and the *Review* represent the crystallization of the scientific work of the Forest Service; they will make possible a very much higher degree of efficiency.

The present number, being the first, is purely preliminary. It gives no conclusions or reports of investigations, but shows the organization and classification of the scientific work of the Forest Service, the problems in need of solution, and, in general, the manner of attacking these problems. It gives the four main heads, Dendrology, Grazing, Products and Silviculture, with their subdivisions, and describes concisely the problems to be studied under each subdivision. Under Dendrology it shows the importance of studies of tree distribution and of wood structure. Under Grazing, work is being done to collect basic information on the forage, to find methods of reseeding the more valuable kinds, both artificially and naturally, and ways of handling stock so as to increase the carrying capacity of the range, better the condition of the stock, and insure complete utilization of the forage. Under Products, investigations are being carried on to learn the properties of wood, mechanical, physical and structural, so that each kind can be put to its best use and handled most efficiently in manufacturing and kiln drying; to increase the knowledge of preservatives, including the methods of using them and their effects; to develop uses for products of trees other than saw

timber, such, for instance, as making alcohol from wood waste; in addition, Products is gathering much statistical information of use not only to the Forest Service, but to all wood-using industries. Products comes in closer contact with the lumber industry than any other branch of the service and has already secured results of great value to lumbermen. Under Silviculture, the *Review* gives in some detail the important problems on which the service is working. It describes briefly the establishment and purpose of the experiment stations; under each head (forestation, forest influences, management, etc.) it not only gives the problems to be studied, but shows their importance and their relation to each other. The experiment being conducted at Wagon Wheel Gap to determine the influence of forest cover on run off and erosion is given rather fully. This is probably the most complete and far-reaching experiment of its kind in the world.

At the end of the *Review* is the investigative program for 1912. A study of this program will show the thoroughness with which the field is being covered.

BARRINGTON MOORE

WASHINGTON, D. C.

SPECIAL ARTICLES

A LABORATORY METHOD OF DEMONSTRATING THE EARTH'S ROTATION

THE two laboratory methods in general use for proving the rotation of the earth are Foucault's pendulum and gyroscope experiments. The first is inapplicable in many laboratories, because there is no convenient place to hang a sufficiently long and heavy pendulum, while the apparatus for the second is necessarily expensive. The following experiment is designed to provide a simple and convenient means by which the earth's rotation may be demonstrated in a small laboratory. The demonstration depends upon the fact that, if a circular tube filled with water is placed in a plane perpendicular to the earth's axis, the upper part of the tube with the water in it is moving toward the east with respect to the lower part. If the tube is quickly rotated through 180 degrees about its east and west diameter as an axis, the part of the tube which was on the upper side attains a relatively westward motion as it is turned downwards (since it is drawing nearer the earth's axis). But the water in this part of the tube retains a large part of its original eastward motion, and this can be detected by suitable means.

Since the east and west axis itself is rotating with the earth, only that component of the water's momentum which is parallel to this axis will have an effect in producing a relative motion when the tube is turned. If then a is the angular velocity of the earth's rotation, r the radius of the circle into which the tube is bent, and θ the angular distance of any small portion of the tube from the east and west axis, the relative velocity between the water and the tube when it is quickly turned from a position perpendicular to the earth's axis through 180 degrees is

$$\text{Velocity} = V = \frac{ar}{\pi}\int_0^{2\pi} \sin^2 \theta d\theta = ar.$$

In order to prevent convection currents, it is best to hold the ring normally in a horizontal position, in which case the relative motion is of course $ar \sin \phi$, where ϕ is the latitude of the experimenter.

To perform the experiment, glass tubing 1.3 cm. inside diameter was bent into a circular ring 99.3 cm. in radius, and a short glass tube closed with a rubber tube and screw

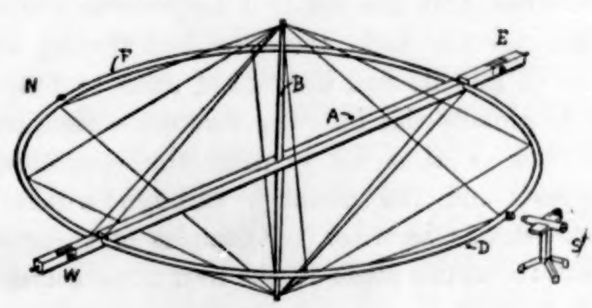

FIG. 1

clamp was sealed into it to allow for the expansion of the water and to provide a place for filling. The ring was fastened with tape into notches in the wooden rod A (Fig. 1), which served as the horizontal axis, and was

supported by wires from the extremities of the cross rod *B*. The ends of the rod *A* were made adjustable perpendicularly to the plane of the ring, so that the ring might be made to swing on an axis parallel to its plane. The ends of the rod were swung in solid supports, adjustable to make the axis horizontal. In order that the motion of the water might be detected, a mixture of linseed oil and oil of cloves of the same density as water was prepared, and a few drops of the mixture were shaken up with the water with which the tube was to be filled. The globules of oil were observed at a point *C*, between the ends of the axis, through a micrometer microscope. Difficulties from the astigmatic refraction of the light by the water in the cylindrical glass tube were overcome by sealing a tubular paraffine cap, closed with a cover-glass and filled with water, on the part of the glass tube under the microscope, thus presenting a plane surface through which to make the observation. One side of the ring was weighted, so that on releasing a catch at the side of the observer the tube swung around through 180 degrees in a definite time, and was held again by the catch just under the microscope.

In taking a reading, the microscope was focused as nearly as possible on the center of the tube, and the ring was left in position until the oil globules had no appreciable motion. As soon as the catch which held the ring in position was released, the time was counted, with the aid of a metronome ticking half-seconds, until the tube had turned and an oil globule had been fixed upon to follow. The globule was followed through a measured length of time by turning the micrometer screw, and the distance through which it moved was recorded. Examples of these observations are given in the first three columns of Table I.

Variations in the readings arose from the fact that the part of the ring toward the east was near a cold wall, so that convection currents were produced as soon as the tube left the horizontal position in making a turn. This effect was made as small as possible by stirring the air with an electric fan. Other variations came from the fact that it was found impossible to adjust the horizontal axis so nearly parallel to the plane of the ring as to prevent a slight effect from turning the tube. Errors from the first cause were corrected by reversing the direction of turning in alternate readings. Those from the latter cause were nullified by taking readings with one side of the ring weighted and then shifting the weight to the other side. In this manner ten readings of each of four different kinds were taken (Cases I., II., III. and IV.), and the fact that the predominant motion is positive, or toward the west as observed on the south side, shows that the earth is turning from the west to the east.

TABLE I

Time from Releasing Catch to Following Water's Motion	Time of Following Water's Motion, Sec.	Distance Through which Water is Followed, Mm.	Time from Completion of Turn to Following Water's Motion, Sec.	Time on Curves of Completion of Turn	Initial Velocity, V, Mm. Sec.$^{-1}$
Case I. Weight on side *D*. Change from heavy to light side.					
7.5 secs.	22.5	+ .40	4.5	+21.2	+.041
7.0	23.0	+ .37	4.0	+22.1	+.03[illegible]
Case II. Weight on side *D*. Change from light to heavy side.					
7.5	22.5	+1.57	4.5	− 1.0	+.160
8.0	22.0	+1.35	5.0	+ .5	+.155
Case III. Weight on side *F*. Change from heavy to light side.					
8.0	22.0	− .59	5.0	+13.4	−.067
7.5	22.5	− .70	4.5	+11.4	−.075
Case IV. Weight on side *F*. Change from light to heavy side.					
7.5	22.5	+ .37	4.5	+19.9	+.045
8.0	22.0	+ .67	5.0	+11.5	+.075

Average V: Case I. = .0434; Case II. = .1580; Case III. = − .0633; Case IV. = .0671.

Calculation of the Initial Velocity

In order to make an accurate estimate of the velocity corresponding to any given reading, the rate of decrease of velocity of the water in the ring must be determined. If the

retardation r is taken to be proportional to the velocity V for this low velocity,

$$r = \frac{dV}{dt} = CV,$$

$$\frac{dV}{V} = Cdt,$$

and

$$\log V = Ct + K$$

will express the value of the velocity at different times. In order to determine the constants C and K, the ring was held in a vertical position until the colder water near the east wall produced a considerable motion. It was then brought back to the horizontal and the time observed which was required to move successive quarter millimeters. A few

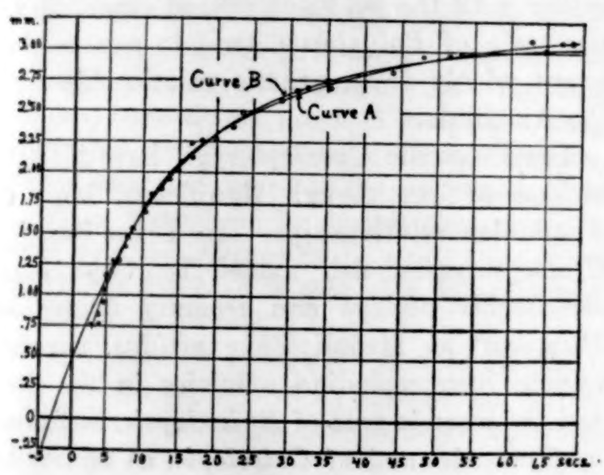

FIG. 2

such readings are given in Table II. From a large number of such observations an average curve was drawn, showing the relation of the distance covered to the time (Fig. 2, Curve A). The slope of this curve was taken at two

TABLE II

	.25	.50	.75	1.00	1.25	1.50	1.75	2.00	Distance in Mm.
1	2	4	7	11	16	22	30	42	Time in seconds.
2		2		5	7	10	15	21	
3	3	7	11	17	23	31	45	68	

of the most definite points, $t = 12.5$ and $t = 30$, and these values were substituted in equation (1) to determine the constants C and K. The curve in Fig. 3 was then drawn from the resulting formula, showing the velocity at any time. Curve B, Fig. 2, was then constructed by integrating this curve graphically with respect to t.

The water in the ring has its maximum velocity just before the turn is completed. The time required to make a complete turn was three seconds, and if this is subtracted from the time in column 1, Table I., it gives the length of time between the completion of the turn and the first observation of the motion (column 4, Table I.). Now if a portion of Curve B (Fig. 2) be taken, such that the distance represented on the curve in the time of any particular reading is the same as the distance in that reading, the beginning of that portion of the curve will correspond to the time at which the motion of the globules

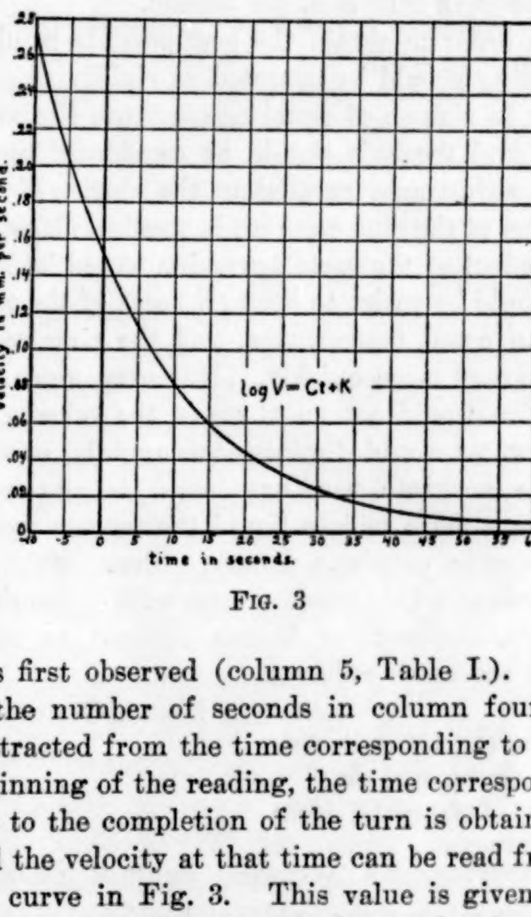

FIG. 3

was first observed (column 5, Table I.). So if the number of seconds in column four is subtracted from the time corresponding to the beginning of the reading, the time corresponding to the completion of the turn is obtained, and the velocity at that time can be read from the curve in Fig. 3. This value is given in column six, and is the velocity at the time of completing the turn. The velocities in each of the four cases are averaged separately, and the average of the four averages is taken as the true motion due to the earth's rotation.

The average of the velocities in these four cases is .0513 mm. per second. From the formula $V = ar \sin \phi$ derived above, we ob-

tain $V = .0484$, a difference of 5 per cent. As a check upon the accuracy of the readings, it will be seen that the differences between the velocities in Cases I. and II. and between those in III. and IV., representing double the velocity due to the difference in density of the water in different parts of the tube, are about equal; also the differences between Cases I. and III., and II. and IV., representing the variation due to imperfect adjustment of the axis, are approximately the same. In order to show that there was no appreciable effect from convection currents while the ring was in a horizontal position, several readings were taken after the tube had remained at rest for some time, none of which showed a motion larger than .015 mm. per second.

In order to obtain the best possible results, the ring should be mounted as rigidly as possible in a room of equal temperature throughout, and the axis should be capable of accurate adjustment parallel to the ring. If the radius of the ring were made smaller, although the effect of the earth's rotation would be less, it would be easier to keep all parts of the tube at an equal temperature, and the ring could be turned more quickly. Moreover, since the motion would not be so great, the velocity of the water would diminish less rapidly, so that more accurate readings could be obtained. With a more mobile liquid the motion would of course continue longer. Even with the comparatively crude apparatus described above, however, it is not difficult to show that the earth revolves.

Arthur Holly Compton
Physical Laboratory,
University of Wooster,
January 13, 1913

CROSSOPTERYGIAN ANCESTRY OF THE AMPHIBIA

For many years evidence has been accumulating for the view that the Amphibia have been derived not from Dipnoi but from Crossopterygians of some sort. Pollard[1] held that the Amphibia were remotely related to the living *Polypterus* and Baur[2] was able to strengthen the evidence, to some extent, from the Stegocephalian side. More recently Thévenin[3] has expressed similar views, while Moodie,[4] correcting Baur's observations on the lateral line grooves in the skull has seemingly demonstrated the general homology of the skull top of *Polypterus* with that of Stegocephalia. Gegenbaur[5] supported the homology of the Stegocephalian cleithrum with the "clavicle" of *Polypterus* and other fishes, while Klaatsch[6] showed that the pectoral limbs of *Polypterus* both in musculature and osteology in many respects remotely suggest Amphibian conditions. On the other hand, Goodrich's[7] studies on the scales of fishes, together with the evidence offered especially by the brain of *Polypterus,* tend to remove that genus widely from genetic relationship with the Amphibia.

The Paleozoic Crossopterygii have hitherto yielded a few, though significant, hints of Amphibian relationship. The Texas Permian Crossopterygian fish named by Cope *Ectosteorhachis nitidus* and recently figured by Hussakof[8] as *Megalichthys nitidus,* suggests remote Stegocephalian affinities in the skull and the same is true of *Rhizodopsis,* as figured by Traquair[9] and of *Osteolepis,* as figured by

[1] "On the Anatomy and Phylogenetic Position of *Polypterus,*" *Zoöl. Jahrb. Abt. f. Anat. u. Ont.* (Spengel), V. Bd., Jena, 1892, pp. 387–428, Taf. 27–30.

[2] "The Stegocephali. A Phylogenetic Study," *Anat. Anz.*, XI. Bd., 1896, No. 22, pp. 657–673.

[3] "Les Plus Anciens Quadrupèdes de France," *Annales de Pal.* (Boule), tome V., 1910, pp. 1–64, pl. I.–IX.

[4] "The Lateral Line System of Extinct Amphibia," *Journ. of Morphol.,* Vol. XIX., No. 2, 1908, pp. 511–540; 1 pl.

[5] "Clavicula und Cleithrum," *Morphol. Jahrb.,* XXIII. Bd., Leipzig, 1895, pp. 1–21.

[6] "Die Brustflosse der Crossopterygier," *Festschr. für Gegenbaur,* I. Bd., 1896, pp. 259–391, Taf. I.–IV.

[7] Cf. Lankester's "Treatise on Zoology," Part IX., first fascicle. "Cyclostomes and Fishes," by E. S. Goodrich, 1909, especially pp. 217–219, 290–300.

[8] "The Permian Fishes of North America," Publ. No. 146 Carnegie Institution of Washington, pp. 168 and pls. 30, 31.

[9] "On the Cranial Osteology of *Rhizodopsis,*" *Trans. Roy. Soc. Edinburgh,* Vol. XXX., 1881.

Pander.[10] *Sauripterus* Hall, a supposed Rhizodont from the Upper Devonian of Pennsylvania has a pectoral girdle and forepaddle which distantly approach Stegocephalian conditions[11] and so also, but in less degree, has *Eusthenopteron.*[12]

Far more definite evidence of the supposed relationship of the Stegocephali with the Paleozoic Crossopterygii has recently been adduced by D. M. S. Watson,[13] of Manchester, in describing some of the large Stegocephalia of the Coal Measures preserved in the Newcastle Museum. He finds that the skulls of the Carboniferous Labyrinthdonts, "*Loxomma*" and *Pteroplax* reveal striking resemblances to the Carboniferous Crossopterygian *Megalichthys:*

> The Basisphenoid of *Megalichthys* has sometimes carotid foramina just as in *Loxomma.* It has small but distinct basi-pterygoid processes which are, however, not provided with articulating surfaces but with sutural ones. The long parasphenoid extends forward to the premaxillæ as it may do in *Pteroplax.* Its lateral borders are in contact with the Pterygoids, to which they afford support, and the bone is connected with the roof of the skull by a fused ethmoid.
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> The pre-vomer is identical with that of "*Loxomma*" in the majority of its attachments, carries one large tusk and a pit for the replacing tooth. It meets its fellow of the opposite side and forms the front of the posterior naris; it is doubtful, however, if it meets the palatopterygoid.
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> The Palatopterygoid of *Megalichthys* is exceedingly like the palatine and pterygoid of *Pteroplax.* They have similar relations to the basisphenoid, parasphenoid and maxilla. There is the same row of small teeth parallel to those of the maxilla with larger teeth inside them, and the pterygoid is covered with the same shagreen of fine teeth.
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> Examination of these primitive and extremely well-preserved skulls seems to show that the ordinary idea of autostylism of the Tetrapoda is incorrect in postulating a connection between the pterygo-quadrate cartilage and the otic region. It is, I think, quite certain that there never was such a connection in primitive forms, except through the dermal bones of the temporal region. The lower attachment with the basisphenoid I have just shown to exist in Crossopterygians, which are hence "amphistylic," in a different way to Notidanus.

The large teeth on the palatine, with infolded bases, exhibit a curious type of tooth replacement which is

> very characteristic of the Stegocephalia, and is unknown elsewhere except in the Crossopterygian fish, where it occurs in a very typical form in the vomerine tusks of *Megalichthys,* and no doubt in many other genera, and in *Lepidosteus.* This occurrence seems to me a strong additional reason for regarding the Tetrapoda as derived from this group of fish.[14]

The lower jaw of "*Loxomma*" likewise approaches the Crossopterygian type in the fact that the splenial is "entirely a bone of the outer side of the jaw as is the first infradentary of the Crossopterygian mandible."

Watson's observations also have important bearing on the relations of the Permian reptilia. These have been distinguished from contemporary Stegocephalians chiefly by the single basi-occipital condyle and by the large pterygoids, which leave only a small interpterygoid vacuity, divided by a narrow parasphenoid. Watson has shown that these and other "reptilian" characters are fully exhibited in "*Loxomma,*" *Pteroplax, Anthracosaurus* and other Carboniferous Stegocephalians, that these characters they share also with *Megalichthys* and that no palate with large vacuities like that of *Eryops* or *Capitosaurus* has ever been found in Carboniferous rocks. He therefore concludes that "the reptilia were separated off very early in the history of the Stegocephalia, preserving features which were

[10] "Ueber die Saurodipterinen, Dendrodonten, Glyptolepiden und Cheirolepiden des Devonischen Systems," 1860, pls. 1–3.

[11] Cf. Gregory, SCIENCE, N. S., Vol. XXXIII., 1911, p. 509. A figure of this forelimb by Hussakof has recently been published by Dr. Bertram Smith in the *Journal of Morphology,* Vol. 23, No. 3, 1912, p. 544.

[12] Cf. Patten, "The Evolution of the Vertebrates," 1912, p. 391.

[13] "The Larger Coal Measure Amphibia," *Mem. and Proc. Manchester Literary and Philos. Soc.,* Vol. LVII., Part I., No. 1, 1912, pp. 1–14, 1 pl.

[14] *Ibid.,* p. 5.

rapidly lost by the latter group, which had a much accelerated evolution." With this conclusion the trend of recent work on the Permian Tetrapoda by Case, Williston, Moodie and Broom, seems to be in accord.

Wm. K. Gregory

American Museum of Natural History

SOCIETIES AND ACADEMIES

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 508th regular meeting was held in the assembly hall of the Cosmos Club, February 22, 1913, with President Nelson in the chair and 76 persons present.

The program consisted of a lecture by Edmund Heller on "Hunting with Rainey in Africa." The communication was chiefly descriptive of the maps and numerous lantern slides exhibited and also of the physical features and vegetation of the country as well as the animals secured during the expedition.

The 509th meeting was held March 8, 1913, with Vice-president Paul Bartsch in the chair and 37 persons present.

Under the heading "Brief Notes, etc.," Wm. Palmer exhibited the head of the small devil ray (*Mobula olfersi*) and a plaster cast made from the same, and explained the feeding habits of this fish. A. C. Weed gave some further account of its habits, and Theodore Gill added some historical notes about devil fishes.

Barton W. Evermann reported results of the sale of blue fox skins from the Pribilof Islands at Lampson's (London) auction of March 7. The 384 skins offered sold at an average price of $56, the highest price being $85.

The regular program consisted of two communications. J. W. Gidley gave an account of a fossil camel recently found in America north of the Arctic circle. The only bone found was a phalanx. The species was an extinct one and its occurrence so far north was regarded as further proof that there once existed land connection between the continents by way of Alaska. The paper was discussed by Messrs. Wilcox, O. P. Hay, Weed, Gill, Evermann, Lyon and others.

The second communication was by Paul Bartsch on "Some Remarkable Philippine Mollusks obtained by the U. S. Bureau of Fisheries Expedition." Specimens of the mollusks described were exhibited by the speaker.

The 510th meeting was held March 22, 1913, with Vice-president Bartsch in the chair and 50 persons present.

Barton W. Evermann reported the executive order of President Taft made March 3, 1913, setting aside the entire chain of the Aleutian Islands as a wild mammal and bird reservation. The reservation is to be under joint charge of the Departments of Agriculture and of Commerce.

A. D. Hopkins announced the recent organization of a new scientific society, The Society for the Advancement of Forest Entomology in America.

The regular program consisted of two communications:

Recent Progress in the Study and Culture of the Common Eel: Hugh M. Smith.

This was a comprehensive outline of the recent discoveries concerning and the completed life history of the common eel. Statistics of the commercial uses of the eel and the methods employed abroad for its propagation and distribution were given. Numerous lantern slides were shown.

Tree-Shrews: Marcus Lyon, Jr.

This paper was based upon a study of many specimens of these squirrel-like insect-eating animals. Of less than 800 known specimens in museums, the British Museum possesses 355, the U. S. National Museum 24, and about 100 are in other collections. The paper was illustrated by lantern slides. Messrs. Bartsch and Wm. Palmer took part in the discussion.

The 511th meeting was held April 5, 1913, with President Nelson in the chair and 43 persons present.

Under the head of Brief Notes, Paul Bartsch reported observations on the habits of the two common toads of the District of Columbia, *Bufo americanus* and *Bufo fowleri*.

Henry Talbott made some remarks on the probable agency of man in the dispersion of animals during the later geological ages. The regular program consisted of two communications:

A Commercial Aspect of Paleontology: by a Layman: Henry Talbott.

The Zoological Results of the Denmark Expedition to Northeast Greenland: Frits Johansen.

The speaker, who accompanied the expedition, gave an account of climatic conditions and the fauna and flora encountered. Mammals and birds received the principal attention. Maps and numerous lantern slides were used.

D. E. Lantz,
Recording Secretary